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ABSTRACT

Setting priorities for allocating funding and other resources is one of the central issues of federal science policy. Presented in these hearings are the views of witnesses (government officials, scientists, economists, and industrial personnel) on various issues related to setting priorities for science. Among the issues addressed (in the form of questions) are: (1) How can the greater public interest be reflected in federal R&D decision-making, both in the choices and in the development and application of technology? (2) How is the optimal funding level of basic research determined? (3) What are the proper roles of government and private industry in funding applied R&D? (4) Should the United States target national economic competitiveness? and (5) What role should state and localities play in determining the direction of scientific research? In addition to transcripts of testimony presented, two supporting papers are provided in appendixes. These are "The Concept of Scientific Choice: A Brief Review of the Literature" by Bruce L. R. Smith and "The Economy of Science: The Proper Role of Government in the Growth of Science" by Simon Rottenberg. (JN)

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SETTING PRIORITIES FOR SCIENCE

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HEARINGS

BEFORE THE

SUBCOMMITTEE ON
SCIENCE, RESEARCH AND TECHNOLOGY

AND THE

SUBCOMMITTEE ON
INVESTIGATIONS AND OVERSIGHT

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

NINETY-SEVENTH CONGRESS

SECOND SESSION

SEPTEMBER 30; DECEMBER 8, 1982

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SETTING PRIORITIES FOR SCIENCE

THURSDAY, SEPTEMBER 30, 1982

HOUSE OF REPRESENTATIVES, COMMITTEE ON SCIENCE AND TECHNOLOGY, SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY, AND THE SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT

Washington, D.C.

The subcommittees met, pursuant to call, at 9:35 a.m., in room 2325, Rayburn House Office Building, Hon. George E. Brown, Jr., presiding.

Mr. BROWN. The subcommittee will come to order.

In the absence of Chairman Walgren and Chairman Gore, who should be here before long, I will take the liberty of opening the hearings this morning because of the large number of distinguished witnesses which we have. I think I should read Chairman Walgren's opening statement to help set the scene for these hearings this morning.

The hearing is being conducted jointly by the Subcommittee on Science, Research and Technology, which he is privileged to chair, and the Subcommittee on Investigations and Oversight, chaired by our distinguished colleague, Congressman Al Gore of Tennessee. We want to express to Mr. Gore our appreciation for suggesting the focus on priorities at this particular time. Setting priorities for allocating funding and other resources in the field of science is as we are all aware one of the central issues of Federal science policy. It is a subject which this subcommittee as well as the full committee has dealt with as a policy issue over the years.

In July 1980, the subcommittee held hearings chaired by Congressman Brown, that is me, on long-range planning for science. In both 1980 and 1981 the full committee held hearings with the American Nobel laureates to hear their views on what is needed to maintain the strength of American science. And early this year Chairman Fuqua held a hearing on the science budget under stress. At that hearing the present and past science advisers to the President discussed with the committee their views of priorities for American science. It is now evident that the science budget will continue to be under stress. As a result, priorities for science will continue to require careful attention in the Congress, in the administration, and in the science, engineering, and industrial community.

The shift from the conditions of significant annual growth in the budget for science to a situation of level or almost level funding has in turn produced new problems and raised new questions. If resources for modest increases in science funding can be found, what area or types of science should be supported? Can funding for some parts of science which are now being supported be reduced or eliminated from the budget? Equally important, what are the best ways to arrive at such decisions.

In the Congress we are thoroughly familiar with the priority setting through the political process which involves geographic claims, lobbyists, and interest groups. We are much less familiar with what I assume is the somewhat more rational processes for priority setting for science which is now employed in industry and in the scientific agencies and by the students of economic theory. We are concerned with the relative emphasis placed on basic research versus applied research and development, and also with how activities such as science and technology education, technology transfer, and science foreign policy, which are closely related to scientific research and development, should be balanced against R&D itself in the resource allocation process.

In this hearing we hope to learn much about both the priorities themselves and the methods for their development. We are grateful to all of our witnesses who have given of their time and experience to help us in this process of self-education.

Without objection we will include a statement from the other cochairman of these hearings, Mr. Gore of Tennessee. I believe we also have an opening statement from the minority.

[The statement follows:]

COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
WASHINGTON, D.C. 20515

Opening Statement

HONORABLE ALBERT GORE, JR.

September 30, 1982

Since World War II, the United States has maintained leadership in many fields of science and technology, especially biomedics and computer technology. We take great pride in these accomplishments which increase our well-being and enhance our culture. The Federal Government has played a key role in the advance of knowledge through long-term real growth in funding for basic and applied research in all scientific areas. In the past decade, accounting for inflation, basic research funding increased by 2 percent, applied research by 3 percent, and development research by 3 percent. Throughout the 1960s, the U.S. ratio of civilian R&D to Gross National Product steadily increased, and after a temporary decline in the early 1970s, surpassed its 1960s levels, reaching an estimated 1.66 in 1981.

Today, economic conditions have caused many to question the role of the United States Government in the promotion of science and technology. The Reagan Administration has stated that federal R&D levels (except defense) are contingent on general economic conditions and fiscal constraint. Indeed, last year federal funding for basic research dropped 2 percent, accounting for inflation. Last year, the Director of the Office of Science and Technology Policy, Dr. Keyworth, told this Committee that in light of the current economic situation, real growth in science cannot continue and that cutbacks in science are not likely to harm the health of American science. Furthermore, OSTP has informed the Congress that R&D will be subject to more vigorous criteria of excellence and pertinence. In that connection, we regret that Dr. Keyworth is not present at this hearing to elaborate on these views.

Other nations have incorporated science advancement as a major component of their industrial policies. Industrialized countries such as Japan & France are subsidizing research and development for new products and services desired in expanding world markets. Such increased international support for science requires even greater U. S. scientific resourcefulness and strategies for maintaining U. S. economic competitiveness. Within this country, as well, increasing competitiveness among the states to attract high-technology industries has resulted in new state science and technology programs for upgrading scientific manpower and encouraging commercial usefulness of university research. Thus, the states will play an increasing role in the direction of science policy.

While we must strive for increased innovation and corporate productivity, the Federal Government must continue to play a vital role in developing non-commercial advances in science which serve the greater public interest by protecting our health, environment, and our national defense. Debate is fast emerging regarding the role of "public interest" as a component in the direction of scientific and technological research. The National Science Foundation's recent

"Five-Year Outlook" emphasizes the stress which science is currently suffering due to the lack of public support for science. We must establish linkages between science and the public in dialogue regarding the role of science in the United States.

Today's hearing will focus on issues vital to this nation's future well-being:

1. How can the greater public interest be reflected in federal R&D decision-making; both in the choices, development and application of technology?
2. How do we determine the optimal funding level of basic research?
3. What are the proper roles of government and private industry in funding applied research and development?
4. Should the United States target certain R&D for international economic competitiveness?
5. What role should state and localities play in determining the direction of scientific research?

We look forward to hearing the views of our distinguished witnesses who represent the views of Federal and State Government officials, scientists, and economists.

Mr. BROWN. Our first panel was originally scheduled to include an additional member, a representative from the Office of Management and Budget. Unfortunately, and I will assume that it is because of the press of last minute business before the Congress, the scheduled witness was unable to appear, and we have been unable to get a substitute, which we very much regret, since obviously the OMB and the Executive Office of the President have a deep involvement in this matter of priority setting. But we will struggle along as best we can under the circumstances.

We had also invited the Director of the Office of Science and Technology Policy, Dr. George Keyworth, or his representative to appear before us. We have instead a thoughtful and useful statement for the record by Dr. Keyworth, and, without objection, it will be inserted in the record at this point.

[The statement referred to follows:]

STATEMENT OF DR. GEORGE A. KEYWORTH, II, SCIENCE ADVISOR TO THE PRESIDENT, AND DIRECTOR, OFFICE OF SCIENCE AND TECHNOLOGY POLICY, EXECUTIVE OFFICE OF THE PRESIDENT FOR THE SUBCOMMITTEES ON INVESTIGATIONS AND OVERSIGHT, AND SCIENCE, RESEARCH AND TECHNOLOGY; COMMITTEE ON SCIENCE AND TECHNOLOGY; HOUSE OF REPRESENTATIVES.
SEPTEMBER 30, 1982

I am pleased to have the opportunity to share with you my views on priority setting *for* science and *in* science. Your attention to this subject is extremely timely, not because it is a new subject, but because it needs new attention. When no limits are placed on resources, then resource allocation is relatively painless and there may be less urgency to set explicit priorities. But it is now time for us to recognize that there are limits to resources, and though the task becomes harder, it also becomes more necessary to define consciously and clearly how we set priorities among all the worthwhile goals competing for finite resources.

I made the distinction earlier between priority setting *for* science and priority setting *in* science. I define the former as the ranking of the pursuit of science as a goal against other national goals, and the latter as the comparative evaluation of different scientific endeavours.

With regard to priority setting for science, this Administration recognizes the importance of maintaining a strong U.S. scientific enterprise. However, this is not a goal separate from and superior to those of a healthy economic growth and a secure national defense. They are highly interdependent. Not only does the economy determine how much R&D the nation can pay for, it also influences what kind of R&D we do.

We strongly believe that the process for setting priorities in science must be adjusted to recognize the reality of limited resources. Greater selectivity must be exercised in funding decisions, and the criteria for exercising such selectivity are excellence and pertinence to the national goals of economic health and national security. The scientific community must also assume more forcefully its responsibilities for setting priorities in science. It must give us the benefit of its thoughtful and critical judgment.

To encourage the scientific community's initiative, I have asked the National Academy of Sciences, earlier this year, to engage the community of American scientists in developing a process that will help us determine, jointly, the fields of science that are of the highest quality, most exciting and with the greatest potential for future applications. I expect the Academy to report on their progress in the near future.

We have solicited the scientific community's recommendations in other ways as well, and have been receptive to them. As one example, last fall, at my request, the High Energy Physics Advisory Panel appointed by the Department of Energy examined the question "What would be necessary to maintain the leadership of the United States in high energy physics?". Based upon their advice, the President's Fiscal 1983 budget recommendation for the DOE high energy physics program reflected the need to ensure optimal use of existing facilities and the ability to meet future demands for new experimental facilities.

I hope that the joint priority setting process that has begun will take on a stronger momentum, and that the scientific community will continue actively to recommend n[illegible]l, those areas for increased support, but also those that must be reduced so that more productive efforts may receive the deserved support.

This Administration has enunciated a clear science policy that the scientific community can use as guidance in setting its priorities. Our science policy aims to define clearly the appropriate role of the Federal government in R&D so that the industrial and academic sectors may proceed to fulfill their roles without the fear of undue government intervention. It would get the government out of the development and demonstration activities, except where the government is a customer. It places priority for Federal funding on those areas that the government does well, such as basic research, and those where it should be the R&D performer because the payoffs are not appropriable by private sector parties.

The Federal government has the major responsibility for making sure that our knowledge base is healthy and continues to grow. As a technological nation, we depend vitally on this base and on the strengths of the universities where basic research is done and where new scientists and engineers are trained. So, we will continue to support high quality basic research. We will seek to maximize the return on the Federal investment by focusing on those areas with potential for eventual applications to our industrial well-being and national security. We are concentrating much of the money available for growth on those areas with the most promise, those where there is bubbling intellectual excitement. Again, high energy physics and astrophysics are examples of such areas. The link to technological applications may be still tenuous here, but the fields are so rich in talent and so full of promise that the adequate allocation of resources is likely to pay off well. Another example, with a stronger link to potential applications, is materials research. New methods developed in recent years to do materials engineering at the atomic level have opened up vastly the potential of this field.

I don't think there is argument about the Federal responsibility for national security--the assurance of a peaceful, stable environment in which to pursue our lives. Unfortunately our national defense has been taken for granted and allowed to deteriorate over the years. While the Soviets were upgrading their capabilities, we failed to adequately exploit our own technological edge, which we have relied upon traditionally for our military strength. So, this Administration has given top priority to rebuilding our defense capacity and upgrading our R&D base for defense.

I have cited excellence and pertinence as two criteria for priority setting. Excellence is well understood--we will emphasize excellence in the investigators, excellence in the subject matters, and excellence in the expected results. Pertinence may be less well understood, particularly as it applies to basic research. To explain the criterion of pertinence for our funding of basic research, I must point out a paradox that we are facing. For at least the past 30 years, the United States has done the world's best basic research and we show no slackening of our lead today. Yet, our lead in high technology industries is being challenged. Industrial competitiveness is a function of economic, regulatory as well as technological factors. We have taken steps to alleviate the regulatory burden. But we must also address a fundamental ill that has developed in this country. This ill is the persistent assumption that the profit system and our private sector are somehow incompatible with good scientific research. Admittedly, the Federal government, through two decades of massive support for research with little attention to its ultimate use, has contributed to this situation. To change it, we must now strengthen the connection between basic research and technological application. We must be assured that basic research is used as much and as often as possible. We must see a strong interchange between our basic researchers and industrial R&D centers. I am not minimizing the pure cultural value of pursuing good science, but let us also recognize that we justify substantial public support for science--even in these times of budget austerity and reduced government intervention--on its promise for tomorrow's well-being and prosperity.

The question of research performed in universities versus elsewhere has been raised. I see three major R&D performing groups: the universities, the Federal laboratories and industrial research centers. In my opinion, they play three distinct roles:

-The Federal laboratories should be funded by the government only for what I call long range applied research. That is the research from which technological developments will spring, but at a sufficiently distant future that industry will not fund it. Federal laboratories should also perform research and development activities appropriate only for Federal performance (e.g. nuclear weapons, food and drug testing). Those Federal laboratories that are endowed with large, sophisticated facilities, such as the national laboratories of the Department of Energy, should make them available to university and industrial researchers.

-Universities should continue to excel in their primary role of education, and should conduct basic research as an essential adjunct to this educational role.

-Industrial research centers should continue what they do much better than the Federal and university sectors: applied research and product development.

Again, I want to stress that even though their roles are distinct, these three sectors must work together. I have asked the White House Science Council (WHSC) to review the activities of the Federal laboratories and to recommend to me their appropriate responsibilities and ways to strengthen them. I am expecting the WHSC to report their findings by next April.

To summarize the question of priority setting for science and in science I will say that it is a joint responsibility of the Federal government and the scientific community. The scientific community must provide its guidance and recommendations to the government, just as the government must formulate a clear science policy that would establish the broad priorities which the scientific community can follow to propose, evaluate and recommend programs and projects. This Administration's science policy has three key elements: maximize the payoff for Federal investment in basic research, improve the flow of knowledge between the research establishment and industry, and strengthen the defense technology base. We look forward now to working with the scientific community to maintain and increase our scientific and technological strength.

Our first panel will include Dr. James B. Wyngaarden, Director of the National Institutes of Health, Dr. Edith Martin, Deputy Under Secretary of Defense for Research and Advanced Technology, and Dr. Donald Langenberg, Deputy Director of the National Science Foundation, all of whom have a great deal to contribute to the focus of these hearings. And so we welcome you, and I presume you may proceed in order in which I have indicated, and I welcome our distinguished chairman who will take over at this point.

Mr. WALGREN. Thank you very much, Mr. Brown. I apologize for my absence, but we are here to create a record that all of us can work with, and so I am glad you proceeded. Please proceed.

STATEMENT OF DR. JAMES B. WYNGAARDEN, DIRECTOR, NATIONAL INSTITUTES OF HEALTH, BETHESDA, MD.

Dr. WYNGAARDEN. Mr. Chairman, members of the subcommittees, I appreciate this opportunity to discuss the setting of priorities in science. As Director of the National Institutes of Health, I will focus today on one aspect of this important topic; namely, priorities in biomedical research.

To be most responsive to the concerns of the subcommittees, I will discuss three major areas of influence on priority setting at NIH: the overall mission of NIH, scientific considerations, and other factors of influence, such as specific public mandates and assignments as expressed by Congress and the administration.

The mission of NIH is a broad one, but it can be stated simply: "to uncover new knowledge that will lead to better health for everyone." That public commitment to biomedical research has been restated in various forms since 1887 when the Hygienic Laboratory was established at the Marine Hospital on Staten Island for research on cholera and other infectious diseases. The mission was reaffirmed in 1930 when Congress passed the Ransdell Act, which established a National Institute of Health.

Congress underscored its faith in the value of health research in 1944 when it consolidated and revised laws relating to the Public Health Service.

The broad intention of Congress in section 301 was emphasized in a House report on the PHS Act:

> Part A of this title would consolidate and restate the basic authority of the Public Health Service in the whole field of research, so as to grant in clear and unmistakable terms, broad authority to carry on investigations through its own personnel, and to cooperate and assist in the investigation by others, of all problems bearing on the physical and mental health of our people.

Confidence in the potential of health research, together with a multibillion-dollar investment, through the Congress, has built a biomedical research capacity second to none. It is a source of national pride and international prestige.

NIH operates a large program of laboratory and clinical research in its own facilities, but four-fifths of the expenditures of the agency go to the support of biomedical research conducted in universities, private laboratories, and elsewhere. The Federal Government contributed nearly 60 percent of support from all sources for health-related research and development in 1981; the NIH budget accounts for about 70 percent of the total Federal investment. From the beginning, the main NIH mechanism of research support has been individual project grants for studies proposed by non-Government scientists. About half of the NIH budget currently supports such investigator-initiated projects.

NIH programs express the national policy shaped by Congress through the years. That policy encourages diversity and excellence in health research; it relies on the scientific community itself to identify, through a competitive peer review process, the ideas and investigators most worth of support. In this way, public funds invested in biomedical research support the most creative ideas and energies in the American health research community.

We also engage in a variety of research-related activities such as clinical trials to test new medical technologies and drugs and devices, community education and demonstrations, and development of orphan drugs. But they all build upon new knowledge generated through fundamental biomedical research.

The ultimate goal of biomedical research is the elimination of disease and disability: the prevention of maladies before they strike or cause damage. To prevent disease, we need to know the process—how the disease acts, and its target in the body. Sometimes we are fortunate, as in the case of polio vaccine, where a confluence of factors plus a determined public health effort removed almost overnight a cloud that had hung over society for many years. That kind of conquest of disease, however, is rare. Most often we slowly amass evidence and carefully look to place each new piece of information in a giant puzzle, in the process creating new opportunities that lead us in previously unforeseen directions.

Fundamental to decisionmaking on research priorities is the availability of a critical mass of basic knowledge with which to launch further inquiries. This point is well illustrated by a brief review of events leading to the newly licensed vaccine to combat hepatitis B virus, the cause of a debilitating liver disease.

Dr. Baruch S. Blumberg, a scientist studying human immune mechanisms with support from NIH, had been pursuing such basic studies for about 13 years when, in 1963, a substance in the blood of an Australian aborigine caught his attention. For a time he thought he had discovered an evolutionary marker in the blood of a primitive people. Some years later, he recognized that this marker was a protein fragment left by a previous infection with hepatitis B. This leap of insight opened up new studies on the structure of the virus and, eventually, led to the development of the vaccine. This much-foreshortened account makes several points concerning the critical importance of basic research.

First, one cannot predict where a basic discovery will have its greatest impact. In this case, an immunologist contributed to infectious disease control.

Second, there may be a long timelag between the initial research and its clinical application; in this case almost 20 years.

Third, one cannot direct an application before the basic knowledge is available. Had planners decided in the mid-1950's to develop a vaccine against hepatitis B, no one would have known where to begin. As Dr. Blumberg said in his address in accepting the Nobel Prize in 1976: "At the outset we had no set views on where this path might lead * * *. I could not have planned the investigation at its beginning to find the cause of hepatitis B."

A corollary to the investigator-initiated approach is the need to assure a continuing supply of well-trained scientists to carry out the research needed to meet national health goals. There is a close and reciprocal relationship between the continued productivity of research and the availability and replenishment of qualified investigators.

All research planning, then, proceeds from what is known about a health problem, life process, or disease area. Such a review leads to the identification of scientific opportunity, gaps, and needs, and rapidly advancing areas of research. We also review potential interventions ready for clinical testing and approaches or emphasis required to advance the knowledge base further. Closely allied is a consideration of the existence of adequate research resources. Modern biomedical research demands sophisticated equipment and instrumentation to maintain high quality of work. The aim is to move along the continuum from basic research to treatment at the bedside to ultimate prevention.

The selection of priority areas for clinical research depends on assessments of:

The physical, emotional, societal, and economic costs of various diseases.

The readiness of the state of knowledge for application to disease prevention or treatment.

The potential impact of various research strategies on reducing the burdens of disease.

That approach to investment in research has paid off in lives saved, in disabilities overcome, and in economic benefits. One economic analysis of the period 1900 to 1975, for example, estimated savings from reduced morbidity and mortality at between $300 and $480 billion. Taking the conservative estimate, this indicates a return of at least $10 for every dollar invested in health research.

In setting research priorities, we also give major consideration to concerns and wishes of the public, expressed directly and through congressional and executive branch actions. Authorizing legislation, mandates, directives, and appropriations all influence our research planning and the conduct of our programs. A few examples of congressional actions that set our course are: concern for providing greater support to specific areas of research or studies; mandates to allocate a proportion of resources to a particular disease area; directions for long-range plans; changes in research training legislation; proposals for small business set-asides; and the establishment of new institutes with specific charges, for example, aging and environmental health sciences.

Additionally, an important part of the planning process involves the views of professional societies and voluntary health organizations, the biomedical research community, and the general public. These views are sought through a variety of means, ranging from structured activities such as National Advisory Councils, special groups or task forces or commissions to consider specific research areas or health problems, to less structured interaction with representatives of such groups as conditions warrant.

Within NIH there are comprehensive and highly organized processes for setting priorities within each Bureau, Institute, and

Division. The priorities are eventually integrated within the Director's office as part of the annual budget request to the Department.

Three decades of vigorous public support of biomedical research have now produced a knowledge base that offers unparalleled opportunity for capitalization by the public and private sectors of the research community. Let me cite some examples:

The development of recombinant DNA technology gives us an exciting tool with which to transfer hereditary units from one species to another. It permits, for example, bacteria to become "factories" that produce substances of biological, agricultural, and medical importance. This technique has already led to the synthetic production of human insulin, somatostatin, and growth hormone. These substances are now being tested in NIH-sponsored clinical trials to determine their effectiveness in treating insulin-dependent diabetes and certain types of dwarfism. Recombinant DNA technology can also yield large quantities of pure antigen which, in turn, may soon be used in vaccines for immunization against infectious agents.

In addition, large quantities of highly specific antibodies can now be produced in the laboratory from hybridomas produced by cell fusion. This process results in monoclonal antibodies that can be used with great precision in research on vaccines, diagnostic tests, and treatments for many diseases. Recently, for example, investigators used human lung cancer cells to prepare monoclonal antibodies that can distinguish human tumor cells from normal cells. This technology might permit the detection of cancer at a very early stage. Eventually, clinicians may be able to attach radioactive or chemotherapeutic agents to the antibodies and thereby kill cancer cells without harming surrounding tissue.

Remarkable progress has been made in understanding the immune system. Scientists have discovered genetic mechanisms that control the immune response to such invaders as cancer cells, transplanted organs, and environmental substances that cause allergies. The genes that regulate these immune responses are called the major histocompatibility complex (MHC). Further knowledge of MHC in relation to many diseases—juvenile-onset diabetes, certain kinds of arthritis, Alzheimer's disease, chronic hepatitis, myasthenia gravis, and others—may lead to better strategies for resisting them and to better techniques for organ and tissue transplantation.

An especially fruitful and expanding area is research in neurobiology. Investigators are finding many substances that have profound effects on the nervous system—endogenous neurotransmitters and neurohormones, as well as many externally applied pharmacologic agents. Such substances act at "receptor sites" in many locations within brain and nerve tissue. Some recently discovered examples are the body's natural painkillers—the endorphins and other opiate-like substances—which may provide fuller understanding of the mechanisms of brain function, the cause of substance abuse or drug addiction, and the treatment of pain. Their discovery has opened a whole new arena of research which may well lead to the conquest of illnesses of the mind and mood, such as depression, that have long baffled medical science.

Two new research instruments—the PETT scanner (positron emission transaxial tomography) and the NMR scanner (nuclear magnetic resonance)—are generally regarded as promising and versatile technologies that provide noninvasive methods for the study of organ structure, function, and metabolism in living subjects. Like the CAT scanner (computer-assisted tomography), they promise to have a major impact on diagnostic medicine in the future.

These scientific advances form the base upon which we will build important achievements and health benefits in the years ahead. They will not come quickly or easily, but there is a momentum to science that gives me confidence in the future. We are closing in, I believe, but we still must understand the fundamental processes and mechanisms of heart disease, cancer, stroke, schizophrenia, arthritis, diabetes, and other major diseases. We must set our priorities in full awareness of the enormous toll of such disorders, their complexity, and the state of knowledge and readiness for discovery that I have outlined.

We recognize and strongly believe that specific research priorities should be set most carefully. We would do the progress of science a disservice if we, as managers, were to become preoccupied with short-term objectives. The power of science is generated by the creative genius of the individual scientist or research group working freely within broad general guidelines. We can best encourage creativity and assure quality by providing adequate resources and prudent guidance.

Thank you for the opportunity to present these views on research priority-setting in biomedical science. I will be pleased to answer any questions you may have.

[The prepared statement of Dr. Wyngaarden follows:]

STATEMENT BY

JAMES B. WYNGAARDEN, M.D.
DIRECTOR
NATIONAL INSTITUTES OF HEALTH
PUBLIC HEALTH SERVICE
DEPARTMENT OF HEALTH AND HUMAN SERVICES

Chairmen Gore and Walgren and Members of the Subcommittees:

I appreciate this opportunity to discuss the setting of priorities in science. As Director of the National Institutes of Health, I will focus today on one aspect of this important topic, namely, priorities in biomedical research.

To be most responsive to the concerns of the Subcommittees, I will discuss three major areas of influence on priority setting at NIH: the overall mission of NIH, scientific considerations, and other factors of influence, such as specific public mandates and assignments as expressed by Congress and the Administration. The three areas interweave, but for purposes of discussion, I have separated them. Finally, I will briefly describe for you the research opportunities now before us.

Mission of NIH

The mission of NIH is a broad one, but it can be stated simply: "to uncover new knowledge that will lead to better health for everyone." That public commitment to biomedical research has been restated in various forms since 1887 when the Hygienic Laboratory was established at the Marine Hospital on Staten Island "for research on cholera and other infectious diseases." The mission was reaffirmed in 1930 when Congress passed the Ransdell Act, which established a National Institute of Health. At that time, the Act's author, Senator Joseph E. Ransdell, saw a broad scope for the new Agency, telling the Senate:

> "This institution should be an international clearinghouse for health. . . . A vast amount of research work is awaiting the attention of scientists in the field of medicine and its application for the alleviation of suffering. . . . Progress in the future may be expected to depend on the advancement of scientists, and that country will be most benefited whose citizens are encouraged to engage in systematic research and aided in doing so."

Congress underscored its faith in the value of health research in 1944 when it consolidated and revised laws relating to the Public Health Service. In Section 301 of the PHS Act it gave a broad charter for the conduct of biomedical research by directing the Surgeon General to:

> ". . . conduct in the Service, and encourage, cooperate with, and render assistance to other appropriate public authorities, scientific institutions, and scientists in the conduct of, and promote the coordination of, research, investigations, experiments, demonstrations, and studies relating to the causes, diagnosis, treatment, control, and prevention of physical and mental diseases and impairments of man. . . ."

The broad intention of Congress in Section 301 was emphasized in a House report on that PHS Act, stating:

> "Part A of this title would consolidate and restate the basic authority of the Public Health Service in the whole field of research, so as to grant, in clear and unmistakable terms, broad authority to carry on investigations through its own personnel, and to cooperate and assist in the investigation by others, of all problems bearing on the physical and mental health of our people."

This expression of confidence in the potential of health research, together with a multi-billion-dollar investment, built a biomedical research capacity second to none. It is a source of National pride and international prestige. I should point out here that, while the Federal role is central and essential, there is no Federal monopoly on health research. In speaking of the Nation's research capacity, we include laboratories in publicly supported and private colleges and universities, voluntary organizations and foundations, and the laboratories of business and industry.

NIH operates a large program of laboratory and clinical research in its own facilities, but four-fifths of the expenditures of the agency go to the support of biomedical research conducted in universities, private laboratories, and elsewhere. The Federal Government contributed nearly 60 percent of support from all sources for health-related research and development in 1981; the NIH budget accounts for about 70 percent of the total Federal investment. From the beginning, the main NIH mechanism of research support has been individual project grants for studies proposed by non-government scientists. About half of the NIH budget currently supports such investigator-initiated projects.

NIH programs express the national policy shaped by Congress through the years. That policy encourages diversity and excellence in health research; it relies on the scientific community itself to identify, through a competitive peer review process, the ideas and investigators most worthy of support. In this way, public funds invested in biomedical research support the most creative ideas and energies in the American health research community--a wealth of ideas whose range far exceeds those that could be generated by any single committee, board, council, or laboratory.

We also engage in a variety of research-related activities, such as clinical trials to test new medical technologies and drugs and devices, community education and demonstrations, and development of orphan drugs. But they all build upon new knowledge generated through fundamental biomedical research. Our mission does not include programs of health services or delivery nor of regulation, but of course, we collaborate with agencies and organizations with such responsibilities, particularly in regard to future research needs from service and regulatory perspectives.

Scientific Considerations in Priority Setting

It is useful to think of research as a continuum stretching from investigations into the most fundamental mysteries of nature to programs designed to translate and then transfer new knowledge to the practicing physician.

The ultimate goal of biomedical research is the elimination of disease and disability: the prevention of maladies before they strike or cause damage. To prevent disease, we need to know the process--how the disease acts, and its target in the body. Sometimes we are fortunate, as in the case of polio vaccine, where a confluence of factors plus a determined public health effort removed almost overnight a cloud that had hung over society for many years. That kind of conquest of disease, however, is rare. Most often we slowly amass evidence and carefully look to place each new piece of information in a giant puzzle, in the process creating new opportunities that lead us in previously unforeseen directions.

The fundamental approach to discovery at NIH continues to be through support of investigator-initiated basic research into the life processes. Biomedical science is increasingly probing the cellular and molecular levels of function. Through competing research projects, we tap the best minds and most creative ideas, weigh them through peer review of substance and methodology, and test them through challenge and open exchange of information. Proposals are further evaluated by advisory councils for program relevance. We believe this approach will produce the ultimate answers, a belief based on the record of the past. In order to maintain the momentum of discovery, we consider it extremely important to continue to place a high priority on the award of new and competing research project grants.

Fundamental to decision-making on research priorities is the availability of a critical mass of basic knowledge with which to launch further inquiries. This point is well illustrated by a brief review of events leading to the newly licensed vaccine to combat hepatitis B virus, the cause of a debilitating liver disease.

Dr. Baruch S. Blumberg, a scientist studying human genetics with support from NIH, had been pursuing such basic studies for about 13 years when, in 1963, a substance in the blood of an Australian aborigine caught his attention For a time he thought he had discovered an evolutionary marker in the blood of a primitive people. Some years later, he recognized that this marker was a protein fragment left by a previous infection with hepatitis B. This leap of insight opened up many new studies on the structure of the virus and, eventually, led to the development of the vaccine. This much-foreshortened account makes several points concerning the critical importance of basic research.

First, one cannot predict where a basic discovery will have its greatest impact. In this case, an immunologist contributed to infectious disease control.

Second, there may be a long time lag between the initial research and its clinical application; in this case almost 20 years.

Third, one cannot direct an application before the basic knowledge is available. Had planners decided in the mid-1950s to develop a vaccine against hepatitis B, no one would have known where to begin. As Dr. Blumberg said in his address in accepting the Nobel Prize in 1976: "At the outset we had no set views on where this path might lead. . . . I could not have planned the investigation at its beginning to find the cause of hepatitis B." Where this work will lead in the future is an open question, but it has important implications--recent research on the hepatitis B virus has suggested an association with liver cancer, the most common form of cancer on a worldwide basis.

A corollary to the investigator-initiated approach is the need to assure a continuing supply of well-trained scientists to carry out the research needed to meet national health goals. There is a close and reciprocal relationship between the continued productivity of research and the availability and replenishment of the supply of qualified investigators. There also exists a special need to replenish the supply of physician investigators and, accordingly, we are making every effort to attract a sufficient number of investigators capable of conducting clinical research.

All research planning, then, proceeds from what is known about a health problem, life process, or disease area. Such a review leads to the identification of scientific opportunity, gaps and needs, and rapidly advancing areas of research. We also review potential interventions ready for clinical testing and approaches or emphases required to advance the knowledge base further. Closely allied is a consideration of the existence of adequate research resources. Modern biomedical research demands sophisticated equipment and instrumentation to maintain high quality of work. The aim is to move along the continuum from basic research to treatment at the bedside.

The selection of priority areas for clinical research depends on assessments of:

--The physical, emotional, societal, and economic costs of various diseases.

--The readiness of the state of knowledge for application to disease prevention or treatment.

--The potential impact of various research strategies on reducing the burdens of disease.

I would like to cite here three examples that, in varying ways, reflect these assessments.

One example is the National High Blood Pressure Education Program of the National Heart, Lung, and Blood Institute, which is based on the knowledge that hypertension is a major factor in stroke and heart disease. In this program, public education on factors that contribute to hypertension is conducted through cooperation with a wide range of public and private organizations and institutions and with business and industry. This program, and the findings of biomedical research, have contributed to a greater public awareness of the importance of nutrition, exercise, non-smoking and other personal practices in the reduction or prevention of high blood pressure.

Other diseases may receive high priority because they provide unique research opportunities despite their relatively low prevalence or limited geographic distribution. These comparatively rare diseases may have well-defined metabolic or genetic characteristics that make it possible to study underlying mechanisms that also occur in more prevalent diseases but are masked by complicating changes.

An interesting example is Huntington's disease, which is a genetic disorder of low prevalence in the United States. However, the National Institute of Neurological and Communicative Disorders and Stroke has given priority to a study of this disorder in collaboration with scientists at a university in

Maracaibo, Venezuela. A population of 200 patients descended from a Spanish sailor who arrived in the country in 1860 is providing a unique opportunity to study an inherited disease with neurologic manifestations that are similar to those in Parkinson's disease. This is a valuable approach because the neurologic manifestations of the more prevalent Parkinson's are less well defined than those in Huntington's disease.

Finally, pursuit of fundamental answers in biomedicine not only leads to new ways to alleviate or control disease and disability, but also to cutting the costs of health care. An example is the practice of treating kidney disease by kidney dialysis and transplantation, great achievements of biomedical research that have restored and prolonged many lives. But this approach is costly and can only compensate for the damage already caused by disease processes we do not fully understand. Therapy of this kind has been termed "halfway technology" by Dr. Lewis Thomas. Consequently, one of the highest priorities of the National Institute of Arthritis, Diabetes, and Digestive and Kidney Diseases is to support research into the major diseases that lead to irreversible renal failure so that we can prevent or arrest them.

The Nation's approach to investment in research has paid off in lives saved, in disabilities overcome, and in economic benefits. One economic analysis of the period 1900 to 1975, for example, estimated savings from reduced morbidity and mortality at between $300 and $480 billion. Taking the conservative estimate, this indicates a return of at least $10 for every dollar invested in health research. (Mushkin, S.J., 1979, "Biomedical Research: Costs and Benefits," Cambridge, Mass., Ballinger, p. 412.)

Other Considerations in Priority Setting

In setting research priorities, we also give major consideration to the concerns and wishes of the public, expressed directly and through Congressional and Executive Branch actions. Authorizing legislation, mandates, directives, and appropriations all influence our research planning and the conduct of our programs. A few examples of Congressional actions that set our course are: concern for providing greater support to specific areas of research or studies; mandates to allocate a proportion of resources to a particular disease area; directions for long-range plans; changes in research training legislation; proposals for small business set-asides; and the establishment of new Institutes with specific charges.

Additionally, an important part of the planning process involves the views of professional societies and voluntary health organizations, the biomedical research community, and the general public. These views are sought through a variety of means, ranging from structured activities such as National Advisory Councils, special groups or task forces or commissions to consider specific research areas or health problems, to less structured interaction with representatives of such groups as conditions warrant.

Within NIH there are comprehensive and highly organized processes for setting priorities within each Bureau, Institute and Division. The priorities are eventually integrated within the Director's office as part of the annual budget process.

Research Opportunities in the 1980's

Three decades of vigorous public support of biomedical research have now produced a knowledge base that offers unparalleled opportunity for capitalization by the public and private sectors of the research community. Let me cite some examples:

The development of recombinant DNA technology gives us an exciting tool with which to transfer hereditary units from one species to another. It permits, for example, bacteria to become "factories" that produce substances of biological, agricultural, and medical importance. This technique has already led to the synthetic production of human insulin, somatostatin, and growth hormone. These substances are now being tested in NIH-sponsored clinical trials to determine their effectiveness in treating insulin-dependent diabetes and certain types of dwarfism. Recombinant DNA technology can also yield large quantities of pure antigen which, in turn, may soon be used in vaccines for immunization against infectious agents.

In addition, large quantities of highly specific antibodies can now be produced in the laboratory from hybridomas produced by cell fusion. This process results in monoclonal antibodies that can be used with great precision in research on vaccines, diagnostic tests, and treatments for many diseases. Recently, for example, investigators used human lung cancer cells to prepare monoclonal antibodies that can distinguish human tumor cells from normal cells. This technology might permit the detection of cancer at a very early stage. Eventually, clinicians may be able to attach radioactive or chemotherapeutic agents to the antibodies and thereby kill cancer cells without harming surrounding tissue.

Remarkable progress has been made in understanding the immune system. Scientists have discovered genetic mechanisms that control the immune response to such invaders as cancer cells, transplanted organs, and environmental substances that cause allergies. The genes that regulate these immune responses are called the major histocompatibility complex (MHC). Further knowledge of MHC in relation to many diseases--juvenile-onset diabetes, certain kinds of arthritis, Alzheimer's disease, chronic hepatitis, myasthenia gravis, and others--may lead to better strategies for resisting them and to better techniques for organ and tissue transplantation.

An especially fruitful and expanding area is research in neurobiology. Investigators are finding many substances that have profound effects on the nervous system--endogenous neurotransmitters and neurohormones, as well as many externally applied pharmacologic agents. Such substances act at "receptor sites" in many locations within brain and nerve tissue. Some recently discovered examples are the body's natural painkillers--the endorphins and other opiate-like substances--which may provide fuller understanding of the mechanisms of brain function, the cause of substance abuse or drug addiction, and the treatment of pain. Their discovery has opened a whole new arena of research which may well lead to the conquest of illnesses of the mind and mood, such as depression, that have long baffled medical science.

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Conclusion

We recognize and strongly believe that specific research priorities should be set most carefully. We would do the progress of science a disservice if we, as managers, were to become preoccupied with short-term objectives. The power of science is generated by the creative genius of the individual scientist or research group. We can best encourage creativity and assure quality by providing adequate resources and prudent guidance.

Thank you for the opportunity to present these views on research priority-setting in biomedical science. I will be pleased to answer any questions you may have.

-fpw-9/28/82

Mr. WALGREN. Thank you very much, Dr. Wyngaarden.

Next, Dr. Edith Martin, Deputy Under Secretary of Defense for Research and Advanced Technology for the Department of Defense. Welcome to the committee. Your written testimony will be made a part of the record automatically. Please proceed.

STATEMENT OF DR. EDITH W. MARTIN, DEPUTY UNDER SECRETARY OF DEFENSE FOR RESEARCH AND ADVANCED TECHNOLOGY, DEPARTMENT OF DEFENSE

Dr. MARTIN. It is a pleasure to be here to testify before the subcommittees of the House Committee on Science and Technology.

I. INTRODUCTION

Good morning. It is a pleasure to be here to testify before these subcommittees of the House Committee on Science and Technology.

In preparing my remarks, I have limited the subject to science and technology issues that demonstrate the high national priority of the Department of Defense science and technology program. Even with this narrowing of the topic, we are still faced with a very complex subject that is difficult to cover in this brief testimony.

I plan to discuss several subjects with you today. Specifically, I will address the need to place a high priority on the DOD science and technology program, the structure of our science and technology program, program formulation, and highlights of the research program.

II. THE NEED TO PLACE A HIGH PRIORITY ON THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM

Many years ago the United States made the decision not to attempt to match our potential adversaries, mainly the Soviet Union and the Warsaw Pact, on a person-for-person, tank-for-tank basis. Instead we decided to depend on our superior technology to give us the needed military advantage. As a result, the Department of Defense is intensely involved with technology development, and it is essential that our science and technology program be energetic, well funded, and responsive to defense needs.

To put things into perspective, let me take a few minutes to describe our science and technology program. Its objectives and goals are to: Offset Soviet numerical superiority; Keep ahead of growing Soviet technical threat; Improve reliability and maintainability; Improve productivity of the industrial base; and Enhance return on investment.

By way of motivation, let us note that the Soviets in the last decade have gone from an RDT&E program that is roughly comparable to ours to one that is about twice as large. The Soviets have more scientists and engineers and they are training more than we are, 300,000 last year versus 60,000 in the United States. From the mid-1960's, as was noted in the opening remarks, to the mid-1970's, our technology base program was level funded. With inflation taken into account, our research purchasing power decreased to about half. In 1976 a policy of real growth was instituted, and since then we have regained only about 30 percent of that lost ground.

We seek to continue real growth until we are convinced we have an adequate technology base.

In summary, we recognize our technological superiority is being challenged as never before, and we intend to pursue growth in the Department of Defense science and technology program to meet that challenge.

Let us examine the structure of the defense program. The DOD science and technology program includes basic research, which is 6.1, exploratory development, which is 6.2, and advanced technology developments, which is 6.3A. The S&T program is vertically organized. In many cases a concept enters the system at the research or 6.1 level where fundamental investigations into the nature of basic physical processes are conducted. In evaluating basic research opportunities we emphasize the potential for eventual military payoff. However, a key feature in our scientific process is always the scientific merit of the proposed research.

Successful completion of a basic research program frequently leads to exploratory development, and this is applied research. These efforts are directed toward the solution of specific military problems. They, by our definition, fall short of advanced technology demonstrations and major development projects.

Advanced technology developments, which is 6.3A, normally follow exploratory development. They are brass board demonstrations. Operational systems are notaccomplished in the S&T program.

In short, the tech-base program is organized to stimulate high-quality research and development projects that support our national defense needs.

Our request for 1983 is $4.3 billion, or approximately 2 percent of the DOD budget. The DOD science and technology program accounts for about 18 percent of the DOD research, development, test, and evaluation (RDT&E) program. And similarly, our basic research program represents about 3 percent of the RDT&E program.

Next I would like to discuss program formulation. The procedure by which we select programs bears on the central theme of these hearings. The overall Department of Defense process is covered in our planning, programing, and budget system or PPBS. This system provides for top-down guidance to the services and the defense agencies. The context is set in a document called the Defense Guidance. The services and defense agencies in turn formulate programs to meet the objectives in the guidance.

Initial efforts are reviewed by the Office of the Secretary of Defense during the program objective memorandum cycle, and a second review is accomplished during the budget cycle. The process is overseen by the Defense Resources Board, which considers many mission areas, only one of which is the DOD science and technology program.

Within the S&T program we are concerned with optimizing investment to insure maximum return on investment in terms of increased military capabilities. Change may be gradual or a step function. Evolutionary programs that are important include aerodynamics, propulsion, meteorology, training, and medical research and development unique to the military environment. Programs that could lead to revolutionary advances are given particular attention. These include; Very high speed integrated circuits; chemical defense technology; directed energy technology; precision guided munitions; advanced materials; and ADA high-order language/environment.

Programs under consideration for future emphasis are: Microelectronics, including fail safe/fault tolerant electronics; advanced software techniques and supercomputers; machine intelligence and robotics; and space based radars, infrared arrays and high power microwaves.

Based on threat information and scenarios that help forecast the nature of future conflicts, the services either develop research objectives or technical objectives which define the areas or types of research and development needed. The services, after considering efforts in the private sector and coordination with each other and selected Government agencies, in establishing program priorities. After final research and development allocations have been made by a top-down process, the services formulate specific programs. These planned programs are then reviewed from bottom to top and ultimately presented to the Office of the Secretary of Defense.

The review of every effort, about 20,000 in total, is inconsistent with our management philosophy. Therefore we limit review to the following: The relevancy of the work; the support of defined thrust areas, the support of joint or triservice efforts; the timing of major military systems and programs; and conformance with Defense Guidance. We constantly strive to improve our decisionmaking and management processes. I have just recently established a triservice task force to study our current policies and procedures for guiding and reviewing the science and technology program.

Last, I would like to present a few highlights of the basic research program. DOD has supported basic research for decades beginning with the establishment of the Office of Naval Research in 1946, with the Army Research Office in 1951, and the Air Force Office of Scientific Research in 1952. These organizations support long-term, high-risk, high-payoff research in fields that relate to the DOD mission. Key objectives of our research program are to reduce the chance of long-term technological surprise and to quickly exploit important breakthroughs. In 1983 our 6.1 budget will be about $800 million. Approximately 45 percent, 20 percent, and 35 percent respectively of DOD research is done in academia, private firms, and our in-house DOD laboratories.

Improved coordination of DOD research programs has been achieved through the efforts of the Defense Committee on Research, (DCOR). DCOR members represent the three services, and DARPA and is chaired by the Director for Research and Technical Information in my office.

An important new initiative is the DOD university research instrumentation program. We are well aware that obsolete equipment is hampering DOD university research. Thus in 1983 we are planning to start a university research instrumentation program at a rate of $30 million per year for a 5-year period. Proposals will be judged on the basis of the defense-related research to be done with the equipment. Our efforts will not cure a longstanding national deficiency, but it is at least a start.

Another important initiative addresses current and projected technical talent shortfalls. This is the service fellowship program. In fiscal year 1983 the services will be supporting more than 100 fellows in defense-related disciplines. The Air Force, Army, and Navy fellowships are targeted to the specific areas outlined in my testimony. Stipends of $12,000 to the individual plus $8,000 to the university are intended to attract the best U.S. students into areas of interest to DOD.

The last research program I will comment on is our small business program. Under the defense small business advanced technology program the DOD announced 100 phase I awards to small businesses in December 1981. These awards were at a level of approximately $50,000 each. They were issued for a 6-month period to support preliminary research and development. As required by Public Law 97-219, we are planning the defense small business innovative development program, and are coordinating with the Small Business Administration to insure that our future small business activities support the defense mission.

In conclusion, let me say there is a clear need to place a high priority on the Department of Defense science and technology program. We have elected to rely on our superior technology to counterbalance the superior expenditures and deployment of equipment and manpower by the Warsaw Pact. The burden is on us to make that policy successful.

There is no doubt that the technological superiority on which we depend is being challenged by our adversaries. Our ability to meet the challenge will depend on the maintenance of a vigorous, broadly based, imaginative defense program in science and technology to address the flow of national defense problems. Our only hope of accomplishing this task is through the continuing achievements resulting from our efforts in defense research and development.

Thank you.

[The prepared statement of Dr. Martin follows:]

A HIGH NATIONAL PRIORITY:

THE SCIENCE AND TECHNOLOGY PROGRAM OF THE DEPARTMENT OF DEFENSE

30 SEPTEMBER 1982

by

DR. EDITH W. MARTIN

DEPUTY UNDER SECRETARY OF DEFENSE
(RESEARCH AND ADVANCED TECHNOLOGY)

I. INTRODUCTION

GOOD MORNING. IT IS A PLEASURE TO BE HERE TO TESTIFY BEFORE THESE SUBCOMMITTEES OF THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY.

IN PREPARING MY REMARKS I HAVE LIMITED THE SUBJECT TO SCIENCE AND TECHNOLOGY ISSUES THAT DEMONSTRATE THE HIGH NATIONAL PRIORITY OF THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM. EVEN WITH THIS NARROWING OF THE TOPIC WE ARE STILL FACED WITH A VERY COMPLEX SUBJECT THAT IS DIFFICULT TO COVER IN THIS BRIEF TESTIMONY.

I PLAN TO DISCUSS SEVERAL TOPICS WITH YOU TODAY. SPECIFICALLY, I WILL ADDRESS THE NEED TO PLACE A HIGH PRIORITY ON THE DOD SCIENCE AND TECHNOLOGY PROGRAM, THE STRUCTURE OF OUR SCIENCE AND TECHNOLOGY PROGRAM, PROGRAM FORMULATION, AND HIGHLIGHTS OF THE RESEARCH PROGRAM.

II. THE NEED TO PLACE A HIGH PRIORITY ON THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM.

THE PRESENT ADMINISTRATION IS COMMITTED TO ADVANCE, APPLY, AND DEPLOY -- MORE SUCCESSFULLY THAN IN THE PAST -- RESEARCH AND TECHNOLOGY RESULTS IN THOSE AREAS OF SCIENCE AND TECHNOLOGY THAT ARE ESSENTIAL TO THE RESTORATION OF OUR NATIONAL DEFENSE CAPABILITY. IN RECENT YEARS, OUR NATIONAL SECURITY HAS BECOME INCREAS-

INGLY DEPENDENT UPON MAINTAINING U.S. SUPERIORITY IN BROAD AREAS OF SCIENCE AND TECHNOLOGY. THE SCIENCE AND TECHNOLOGY PROGRAMS OF THE DEPARTMENT OF DEFENSE, AUGMENTED BY THE EFFORTS OF OTHER FEDERAL AGENCIES AND THE PRIVATE SECTOR, PROVIDE THE FOUNDATION FOR HIGHLY EFFECTIVE ARMED FORCES EQUIPPED WITH TECHNOLOGICALLY SUPERIOR WEAPONS SYSTEMS. CONSIDERING THE SUBSTANTIAL QUANTITIVE AND QUALITATIVE ADVANCES IN MILITARY CAPABILITIES OF THE SOVIET UNION AND ITS ALLIES OVER THE LAST DECADE, IT IS IMPERATIVE THAT WE CONTINUE TO INCREASE INVESTMENT IN THOSE AREAS OF SCIENCE AND TECHNOLOGY NECESSARY TO OFFSET THE SOVIET GAINS.

MANY YEARS AGO THE UNITED STATES MADE THE DECISION NOT TO ATTEMPT TO MATCH OUR POTENTIAL ADVERSARIES, MAINLY THE SOVIET UNION AND THE WARSAW PACT, ON A PERSON-FOR-PERSON, TANK-FOR-TANK BASIS. INSTEAD WE DECIDED TO DEPEND ON OUR SUPERIOR TECHNOLOGY TO GIVE US THE NEEDED MILITARY ADVANTAGE. AS A RESULT, THE DEPARTMENT OF DEFENSE IS INTENSELY INVOLVED WITH TECHNOLOGY DEVELOPMENT AND IT IS ESSENTIAL THAT OUR SCIENCE AND TECHNOLOGY PROGRAM BE ENERGETIC, WELL-FUNDED AND RESPONSIVE TO OUR DEFENSE NEEDS.

TO PUT THINGS INTO PERSPECTIVE, LET ME TAKE A FEW MINUTES TO DESCRIBE OUR SCIENCE AND TECHNOLOGY PROGRAM. ITS OBJECTIVES AND GOALS ARE TO:

- OFFSET SOVIET NUMERICAL SUPERIORITY
- KEEP AHEAD OF GROWING SOVIET TECHNICAL THREAT
- IMPROVE RELIABILITY AND MAINTAINABILITY
- REDUCE COST
- IMPROVE PRODUCTIVITY OF THE INDUSTRIAL BASE
- ENHANCE RETURN ON INVESTMENT

ESTIMATES OF SOVIET MILITARY RESEARCH, DEVELOPMENT, TEST AND EVALUATION EXPENDITURES INDICATE THAT THEY EXCEEDED ANNUAL U.S. EXPENDITURES DURING EACH OF THE PAST TEN YEARS. IN THE LAST DECADE THE SOVIETS HAVE GONE FROM

A MILITARY RESEARCH, DEVELOPMENT, TEST AND EVALUATION PROGRAM ROUGHLY COMPARABLE TO OURS TO ONE THAT IS ABOUT TWICE AS LARGE AS OURS AND INCREASING MORE RAPIDLY THAN OURS. THERE IS CONCERN THAT WE MAY BE FALLING BEHIND IN CERTAIN TECHNOLOGIES CRITICAL TO NATIONAL DEFENSE. IN ADDITION, THE SOVIETS HAVE MORE SCIENTISTS AND ENGINEERS WORKING ON THEIR MILITARY EFFORTS, AND ARE TRAINING MANY MORE THAN WE ARE.

IT IS IMPORTANT TO POINT OUT THAT THESE COMPARISONS OF SOVIET AND U.S. SPENDING DO NOT INCLUDE SOME SIGNIFICANT CONSIDERATIONS. WE HAVE A VIGOROUS AND SUBSTANTIAL EFFORT IN PRIVATE SECTOR RESEARCH AND DEVELOPMENT WHICH HAS NO COUNTERPART IN THE SOVIET UNION. INDUSTRY ALSO SPENDS ABOUT 3 BILLION DOLLARS A YEAR ON ITS INDEPENDENT RESEARCH AND DEVELOPMENT (IR&D) PROGRAM, MUCH OF WHICH DOES ADVANCE MILITARY AS WELL AS ASSOCIATED CIVILIAN TECHNOLOGY. ABOUT 32 PERCENT OF THE INDUSTRY IR&D EFFORT IS REIMBURSED BY THE DEPARTMENT OF DEFENSE. AN IMPORTANT EFFORT IS TO MAKE THE DOD SCIENCE AND TECHNOLOGY PROGRAM AND THE INDUSTRY IR&D PROGRAM MORE COMPLEMENTARY AND PRODUCTIVE WITHOUT DISTURBING THE MOST IMPORTANT LETTER IN ITS ACRONYM - THE I FOR INDEPENDENT. OUR ALLIES ALSO TEND TO BE TECHNICALLY ADVANCED, WHEREAS THE SOVIET UNION HAS ALLIES WHO ARE RELATIVELY TECHNICALLY INFERIOR. THUS, A COMPARISON OF MILITARY RDT&E BUDGETS OMITS CERTAIN IMPORTANT ASPECTS OF OUR TECHNOLOGICAL STATURE.

FROM THE MIDDLE SIXTIES TO THE MIDDLE SEVENTIES, THE TECHNOLOGY BASE PROGRAM WAS LEVEL-FUNDED IN CURRENT YEAR DOLLARS. WHEN INFLATION IS TAKEN INTO ACCOUNT, PURCHASING POWER ACTUALLY DECREASED TO ABOUT HALF. STARTING IN 1976 WE INSTITUTED A POLICY OF ANNUAL REAL GROWTH IN OUR RESEARCH AND EXPLORATORY DEVELOPMENT PROGRAMS AIMED AT REGAINING THE APPROXIMATE LEVEL OF THE MID-SIXTIES. WE HAVE SUCCEEDED IN REGAINING ABOUT 30% OF THE LOST GROUND.

IN SHORT, THERE IS NO DOUBT THAT THE TECHNOLOGICAL SUPERIORITY UPON WHICH WE DEPEND FOR OUR SECURITY IS BEING CHALLENGED AS NEVER BEFORE BY OUR ADVERSARIES. WE INTEND TO CONTINUE TO SEEK REAL GROWTH UNTIL WE ARE CONVINCED WE HAVE AN ADEQUATE TECHNOLOGY BASE. THERE IS A CLEAR NEED TO PLACE AN EVEN HIGHER PRIORITY ON THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM.

III. STRUCTURE OF THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM.

THE DOD SCIENCE AND TECHNOLOGY PROGRAM INCLUDES RESEARCH (6.1), EXPLORATORY DEVELOPMENT (6.2) AND ADVANCED TECHNOLOGY DEVELOPMENTS (6.3A) OF THE THREE MILITARY DEPARTMENTS AND THE DEFENSE AGENCIES (DEFENSE ADVANCED RESEARCH PROJECTS AGENCY, DEFENSE NUCLEAR AGENCY, AND THE UNIFORMED SERVICES UNIVERSITY OF THE HEALTH SCIENCES). OUR REQUEST FOR FY 1983 IS $4.3 BILLION, OR APPROXIMATELY TWO PERCENT OF THE TOTAL DOD BUDGET.

THE DOD SCIENCE AND TECHNOLOGY PROGRAM ACCOUNTS FOR ABOUT 18 PERCENT OF THE DOD RESEARCH, DEVELOPMENT, TEST AND EVALUATION (RDT&E) PROGRAM. SIMILARLY, DOD RESEARCH REPRESENTS ABOUT THREE PERCENT OF THE RDT&E PROGRAM. THE TOTAL DOD RDT&E PROGRAM MAKES UP APPROXIMATELY 50 PERCENT OF THE ENTIRE FEDERALLY SPONSORED RESEARCH AND DEVELOPMENT PROGRAM.

THE SCIENCE AND TECHNOLOGY PROGRAM IS VERTICALLY ORGANIZED. A CONCEPT ENTERS THE SYSTEM USUALLY AT THE RESEARCH (6.1) LEVEL, WHERE FUNDAMENTAL INVESTIGATIONS INTO THE NATURE OF BASIC PHYSICAL PROCESSES WITH A POTENTIAL RELATIONSHIP TO A MILITARY FUNCTION OR OPERATION ARE CONDUCTED. THUS, IN EVALUATING RESEARCH OPPORTUNITIES WE EMPHASIZE THE POTENTIAL FOR EVENTUAL MILITARY PAYOFF; HOWEVER, A KEY FEATURE IN OUR SCIENTIFIC SELECTION PROCESS IS ALWAYS

THE SCIENTIFIC MERIT OF THE PROPOSED RESEARCH. SUCCESSFUL COMPLETION OF A RESEARCH (6.1) PROGRAM FREQUENTLY LEADS TO EXPLORATORY DEVELOPMENT (6.2) OR APPLIED RESEARCH. OUR EXPLORATORY DEVELOPMENT PROGRAM INCLUDES ALL EFFORT DIRECTED TOWARD THE SOLUTION OF SPECIFIC MILITARY PROBLEMS, SHORT OF ADVANCED TECHNOLOGY DEMONSTRATIONS AND MAJOR DEVELOPMENT PROJECTS. THIS TYPE OF EFFORT MAY VARY FROM FAIRLY FUNDAMENTAL APPLIED RESEARCH TO QUITE SOPHISTICATED BREAD-BOARD HARDWARE, STUDY, PROGRAMMING AND PLANNING EFFORTS. THE DOMINANT CHARACTERISTIC OF THIS CATEGORY OF EFFORT IS THAT IT BE DIRECTED TOWARD SPECIFIC MILITARY PROBLEM AREAS WITH A VIEW TOWARD DEVELOPMENT. IT USUALLY INVOLVES EVALUATING THE FEASIBILITY AND PRACTICABILITY OF PROPOSED SOLUTIONS AND DETERMINING THEIR PARAMETERS.

EXPLORATORY DEVELOPMENT IS SOMETIMES CALLED THE "BREADBOARD" STAGE. ADVANCED TECHNOLOGY DEVELOPMENTS (6.3A) FOLLOW AND "BRASS BOARD" DEMONSTRATIONS (NOT OPERATIONAL SYSTEMS) ARE ACCOMPLISHED.

A KEY STEP IN THE DOD RDT&E PROGRAM IS ENGINEERING DEVELOPMENT (6.4). I WILL NOT EMPHASIZE ENGINEERING DEVELOPMENT SINCE IT IS NOT PART OF THE DOD SCIENCE AND TECHNOLOGY PROGRAM; HOWEVER, FOR COMPLETENESS IT IS IMPORTANT TO STATE THAT ENGINEERING DEVELOPMENT (6.4) RECONFIGURES THE SUCCESSFUL PROTOTYPE OR "BRASS BOARD" IN PREPARATION FOR PRODUCTION, SHOULD CIRCUMSTANCES REQUIRE IT.

THE VERTICAL ORGANIZATION OF THE DOD SCIENCE AND TECHNOLOGY PROGRAM FOSTERS THE SELECTION OF RESEARCH AND DEVELOPMENT EFFORTS THAT WILL ENSURE OUR MILITARY STRENGTH AND READINESS. IN ADDITION, OUR PROGRAM IS STRUCTURED SO THAT SCIENTIFIC MERIT AND TECHNICAL MERIT ARE KEY FEATURES OF ALL EFFORTS

IN EACH STAGE OF RESEARCH AND DEVELOPMENT. IN SHORT, THE DOD SCIENCE AND TECHNOLOGY PROGRAM IS ORGANIZED TO SUPPORT OUR NATIONAL DEFENSE NEEDS AND TO STIMULATE HIGH-QUALITY RESEARCH AND DEVELOPMENT EFFORTS.

IV. PROGRAM FORMULATION

I BELIEVE A QUESTION OF IMPORTANCE TO THIS COMMITTEE IS THE PROCEDURE BY WHICH WE SELECT PROGRAMS TO BE UNDERTAKEN IN THE DOD SCIENCE AND TECHNOLOGY PROGRAM. THE OVERALL DOD PROCESS IS COVERED BY OUR PLANNING, PROGRAMMING AND BUDGET SYSTEM WHICH IS COMMONLY CALLED THE PPBS SYSTEM. BASICALLY, THE SYSTEM PROVIDES FOR TOP-DOWN GUIDANCE TO THE SERVICES AND DEFENSE AGENCIES. THE SERVICES AND DEFENSE AGENCIES IN TURN FORMULATE PROGRAMS TO MEET THE OBJECTIVES OR EXPLAIN WHY THE DEFENSE GUIDANCE COULD NOT BE MET. THEIR INITIAL EFFORTS ARE REVIEWED BY THE OFFICE OF THE SECRETARY OF DEFENSE (OSD), FIRST, DURING THE PROGRAM OBJECTIVE MEMORANDUM (POM) CYCLE AND, SECOND, DURING THE BUDGET CYCLE. DURING THESE REVIEWS THE PROGRAMS SUBMITTED TO OSD ARE OPTIMIZED TO MEET OUR NATIONAL SECURITY NEEDS. THE OVERALL PROCESS IS OVERSEEN BY THE DEFENSE RESOURCES BOARD (DRB).

THE SCIENCE AND TECHNOLOGY PROGRAM IS ONE OF THE MISSION AREAS CONSIDERED BY THE DRB IN ITS DELIBERATIONS. WE HAVE AN OPPORTUNITY TO PARTICIPATE IN THE TOP-DOWN GUIDANCE PREPARATION AND TO REVIEW THE PRODUCTS GENERATED BY THE SERVICES AND DEFENSE AGENCIES. THUS, SCIENCE AND TECHNOLOGY PROGRAM FORMULATION INTERACTS WITH PREPARATION OF THE TOTAL DEFENSE BUDGET.

WITHIN THE SCIENCE AND TECHNOLOGY PROGRAM WE ARE CONCERNED WITH OPTIMIZING INVESTMENT TO ENSURE MAXIMUM RETURN, IN TERMS OF INCREASED MILITARY CAPABILI-

TIES, FROM OUR RESOURCES. THE PROJECTS UNDERTAKEN FALL INTO TWO GENERAL CATEGORIES. FIRST, ARE THE PROGRAMS THAT GENERALLY ARE EVOLUTIONARY IN NATURE BUT IN WHICH IT IS IMPORTANT THAT ADVANCEMENTS BE MADE. THESE INCLUDE AERODYNAMICS, PROPULSION, METEOROLOGY, TRAINING AND EDUCATION, COMMUNICATIONS AND MEDICAL R&D UNIQUE TO THE MILITARY. THESE DISCIPLINES SHOULD NOT BE CONSIDERED AS SECOND PRIORITY BUT RATHER MORE MATURE AND LESS LIKELY TO HAVE REVOLUTIONARY ADVANCES.

OUR SECOND CATEGORY INVOLVES PROJECTS THAT HAVE A POTENTIAL FOR REVOLUTIONARY ADVANCES IN MILITARY CAPABILITIES. WE DESIGNATE THESE FOR INCREASED MANAGEMENT AND FISCAL EMPHASIS. AMONG OUR CURRENT PROGRAMS THAT FALL IN THIS CATEGORY ARE:

VERY HIGH SPEED INTEGRATED CIRCUITS
CHEMICAL DEFENSE TECHNOLOGY
DIRECTED ENERGY TECHNOLOGY
PRECISION GUIDED MUNITIONS
ADVANCED MATERIALS
ADA HIGH ORDER LANGUAGE/ENVIRONMENT

PROGRAMS OF THIS NATURE ARE GIVEN PARTICULARLY STRONG MANAGEMENT ATTENTION. THEY ARE HIGH PAYOFF ITEMS AND ARE MANAGED IN A VERTICAL SENSE AT THE OFFICE OF THE SECRETARY OF DEFENSE LEVEL. THE LIST IS NOT STATIC. TECHNOLOGIES ARE CONSTANTLY CHANGING AND IT IS OUR GOAL TO SELECTIVELY SEEK OUT AND INITIATE NEW THRUST AREAS. WE USE IN-HOUSE AND OUT-HOUSE ADVICE IN DETERMINING WHAT AREAS TO UNDERTAKE. AMONG THE CANDIDATES UNDER CONSIDERATION ARE:

- MICROELECTRONICS, INCLUDING FAIL SAFE/FAULT TOLERANT ELECTRONICS
- ADVANCED SOFTWARE TECHNIQUES AND SUPERCOMPUTERS
- MACHINE INTELLIGENCE AND ROBOTICS
- SPACE BASED RADARS, INFRARED ARRAYS AND HIGH POWER MICROWAVES

THE LIST IS LONGER AND OF COURSE RESOURCES ARE LIMITED. HOWEVER, IN FY 1984 WE PLAN TO INITIATE A MAJOR SOFTWARE TECHNOLOGY INITIATIVE. WE HAVE MADE CONSIDERABLE PROGRESS TOWARD IMPLEMENTING A STANDARD HIGH ORDER LANGUAGE KNOWN AS ADA, BUT MUCH MORE NEEDS TO BE DONE. THE SYSTEMS USED BY OUR FIGHTING FORCES ARE BECOMING INCREASINGLY MORE DEPENDENT ON COMPUTERS FOR THEIR SUCCESSFUL OPERATION. THIS HAS LED TO BURGEONING SOFTWARE COSTS IN OUR MILITARIZED COMPUTER SYSTEMS. IT INCLUDES NOT ONLY THE ORIGINAL DEVELOPMENT COSTS BUT ALSO THE LIFE CYCLE COSTS WHICH OFTEN SPAN A PERIOD OF TWENTY YEARS. IN ADDITION, SOFTWARE TRANSPORTABILITY (FROM ONE SYSTEM TO ANOTHER) NEEDS TO BE IMPROVED. I AM SETTING UP A SEPARATE OFFICE TO MANAGE AND DIRECT A TRI-SERVICE PROGRAM IN THIS IMPORTANT AREA.

WE DO NOT HAVE WITHIN THE SERVICES A STANDARDIZED METHOD OF FORMULATING THE SCIENCE AND TECHNOLOGY PROGRAM. HOWEVER, EACH SERVICE USES RELATIVELY SIMILAR TECHNIQUES. BASED ON SCENARIOS THAT FORECAST THE NATURE OF FUTURE CONFLICTS, THEY DEVELOP EITHER RESEARCH OBJECTIVES OR TECHNICAL OBJECTIVES WHICH DEFINE THE AREAS OR TYPES OF RESEARCH AND DEVELOPMENT ESSENTIAL TO MEET FUTURE NEEDS. PLEASE NOTE THAT I DID NOT SAY REQUIREMENTS. THE REQUIREMENTS DOCUMENT IS THE RESULT OF A MORE FORMAL PROCESS EMPLOYED BEFORE MORE RESEARCH AND DEVELOPMENT IS UNDERTAKEN ON SPECIFIC SYSTEMS AND EQUIPMENT. THE SERVICES IN TURN PRIORITIZE THEIR EFFORTS WITHIN THEIR EXPECTED RESOURCES, AND AFTER CONSIDERATION OF WORK BEING DONE BY OTHER SERVICES, OTHER GOVERNMENT AGENCIES

AND THE PRIVATE SECTOR. WHEN A FINAL DECISION IS MADE ON RESOURCE ALLOCATION, HERE AGAIN A TOP-DOWN PROCESS, THE SERVICES THEN FORMULATE THE SPECIFIC PROGRAM. THIS IS COMPOSED AND REVIEWED RATHER FORMALLY FROM BOTTOM-TO-TOP AND IS PRESENTED TO THE OFFICE OF THE SECRETARY OF DEFENSE.

AT OSD WE DO NOT ATTEMPT TO APPROVE EVERY PARTICULAR EFFORT. IT IS INCONSISTENT WITH OUR MANAGEMENT PHILOSOPHY AND IMPRACTICAL TO DO IN LIGHT OF THE SIZE OF THE PROGRAM. THE DETAILS CAN BEST BE EVALUATED AT THE LABORATORY LEVEL AND THE SMALL SIZE OF OUR STAFF PRECLUDES LOOKING AT ABOUT 20,000 EFFORTS. WHAT WE DO REVIEW, AND IT IS IMPORTANT, IS: THE RELEVANCY OF THE WORK; THE SUPPORT OF USDRE-DEFINED THRUST AREAS; THE SUPPORT OF JOINT OR TRI-SERVICE EFFORTS; THE TIMING OF MAJOR PROGRAMS; AND, CONFORMANCE WITH DEFENSE GUIDANCE PREVIOUSLY PROVIDED. OFFICE OF THE SECRETARY OF DEFENSE REVIEWS PROVIDE THE BASIS FOR THE FINAL DECISIONS ON THE SCIENCE AND TECHNOLOGY PROGRAM WHICH IS THEN SUBMITTED TO CONGRESS.

FINALLY, WE ARE ALWAYS SEEKING TO IMPROVE OUR OVERSIGHT AND ALLOCATION PROCESS. AT THE PRESENT TIME I HAVE ESTABLISHED A TRI-SERVICE TASK FORCE TO REVIEW OUR CURRENT PROCEDURES FOR REVIEWING THE SCIENCE AND TECHNOLOGY PROGRAM. IT IS IMPORTANT THAT WE CONSTANTLY IMPROVE OUR METHODOLOGY AND PROCEDURE FOR ACCOMPLISHING THIS TASK WITHOUT BECOMING OVERLY COMPLEX IN OUR ADMINISTRATIVE PROCEDURES. IT IS ALSO IMPORTANT THAT THE PROCEDURES ADOPTED ALSO BE ACCEPTABLE TO THE SERVICES IN ORDER THAT WE ACHIEVE WHOLEHEARTED COOPERATION. I EXPECT THE TASK FORCE WILL COMPLETE ITS WORK EARLY NEXT YEAR.

V. HIGHLIGHTS OF THE RESEARCH PROGRAM

THE RESEARCH (6.1) PORTION OF THE DOD SCIENCE AND TECHNOLOGY PROGRAM IS ABOUT $800 MILLION IN THE DEPARTMENT OF DEFENSE AUTHORIZATION ACT OF 1933 (16 Aug 82). THE TOTAL NATIONAL EXPENDITURE FOR BASIC RESEARCH IS $9 BILLION. UNIVERSITIES, INDUSTRIES AND GOVERNMENT LABORATORIES PERFORM ABOUT 45%, 20% and 35%, RESPECTIVELY, OF DOD RESEARCH. OF COURSE, THESE PERCENTAGES VARY SLIGHTLY FROM YEAR-TO-YEAR, LARGELY IN RESPONSE TO THE CHANGING RESEARCH NEEDS OF THE DEPARTMENT OF DEFENSE.

THE DOD HAS SUPPORTED RESEARCH FOR DECADES. WHEN THE CONGRESS ESTABLISHED THE OFFICE OF NAVAL RESEARCH (ONR) IN 1946, DOD BECAME THE FIRST GOVERNMENT ORGANIZATION TO SUPPORT RESEARCH FORMALLY. FOLLOWING THAT THE NATIONAL SCIENCE FOUNDATION WAS ESTABLISHED IN 1950. THE DOD COMMITTMENT TO RESEARCH CONTINUED WITH THE ESTABLISHMENT OF THE ARMY RESEARCH OFFICE (ARO) IN 1951 AND THE AIR FORCE OFFICE OF SCIENTIFIC RESEARCH (AFOSR) IN 1952. THE PROGRAMS AT ONR, ARO AND AFOSR SUPPORT LONG-TERM, HIGH-RISK, HIGH-PAYOFF RESEARCH IN FIELDS THAT RELATE TO THE DOD MISSION. THESE PROGRAMS OF RESEARCH ARE ESSENTIAL TO REDUCE THE CHANCE OF LONG TERM TECHNOLOGICAL SURPRISE AND ALLOW US TO EXPLOIT IMPORTANT BREAKTHROUGHS QUICKLY. IN RECENT YEARS, THE PROGRAMS OF RESEARCH AT THESE DOD ORGANIZATIONS HAVE INCLUDED RESEARCH AREAS THAT REQUIRE CONCENTRATED EFFORTS OVER A PERIOD OF SEVERAL YEARS. THESE SPECIAL PROGRAMS ARE KNOWN AS SPECIAL FOCUS PROGRAMS OR RESEARCH INITIATIVES. WE WILL CONTINUE TO STRIVE FOR SIGNIFICANT GROWTH IN OUR BASIC RESEARCH PROGRAMS. WE ARE COMMITTED TO THIS. IT IS ESSENTIAL.

THE OBVIOUS FOUNDATION FOR THE VERTICALLY STRUCTURED DOD SCIENCE AND TECHNOLOGY PROCESS IS THE 6.1 PROGRAM--HENCE THE RENEWED EMPHASIS ON MANAGING THE PROGRAM FOR GREATEST EFFECTIVENESS. AS A STEP TOWARD THIS GOAL, THE DEFENSE COMMITTEE ON RESEARCH (DCOR) WHICH HAS MEMBERS FROM THE THREE SERVICES, THE DEFENSE ADVANCED RESEARCH PROJECTS AGENCY (DARPA), AND IS CHAIRED BY THE DOD DIRECTOR FOR RESEARCH AND TECHNICAL INFORMATION, SERVES AS A COORDINATING AND POLICY-MAKING GROUP.

OBSOLETE EQUIPMENT IS HAMPERING DOD UNIVERSITY RESEARCH AND WE ARE ACTIVELY ADDRESSING THIS PROBLEM. THE DOD HAS COMMITTED $3 MILLION FOR UNIVERSITY RESEARCH EQUIPMENT TO JOINT SERVICES ELECTRONICS PROGRAMS FOR FY 1982. IN FY 1983 WE ARE PLANNING TO START A BROADER UNIVERSITY RESEARCH INSTRUMENTATION PROGRAM WITH $30 MILLION PER YEAR FOR A FIVE-YEAR PERIOD. THE ARMY RESEARCH OFFICE, THE OFFICE OF NAVAL RESEARCH AND THE AIR FORCE OFFICE OF SCIENTIFIC RESEARCH WILL EACH FUND APPROXIMATELY $10 MILLION IN UNIVERSITY RESEARCH EQUIPMENT IN FY 1983. PROPOSALS SUBMITTED UNDER THIS PROGRAM WILL BE JUDGED ON THE BASIS OF THE PROPOSED DEFENSE RELATED RESEARCH TO BE DONE WITH THE EQUIPMENT. THAT IS, OUR KEY SELECTION CRITERIA ARE THE QUALITY OF THE ASSOCIATED RESEARCH AND THE IMPORTANCE OF THE RESEARCH TO THE TOTAL DOD EFFORT. WHILE THIS DOES NOT CURE A LONG-STANDING DEFICIENCY, IT IS AT LEAST A START, AND ONE THAT I FEEL WILL LEAD OTHER FUNDING AGENCIES TO SIMILAR ACTION. ON A STILL BROADER SCALE, THE DEPARTMENT HAS INCREASED EQUIPMENT BUYS IN ITS CONTRACT PROGRAMS FROM 4 PERCENT IN FY 1976 TO ABOUT 10 PERCENT IN FY 1982. THE PROBLEM OF OBSOLETE RESEARCH INSTRUMENTATION IS BY NO MEANS LIMITED TO DOD RESEARCH PROGRAMS; HOWEVER, WE HAVE A RESPONSIBILITY TO CONTRIBUTE OUR SHARE TO THE SOLUTION OF THIS PROBLEM.

THE ARMY, NAVY AND AIR FORCE HAVE ALL INITIATED FELLOWSHIP PROGRAMS, AND IN FY 1983 WILL BE SUPPORTING MORE THAN 100 FELLOWS IN DEFENSE RELATED DISCIPLINES. THE AIR FORCE FELLOWSHIP PROGRAM FOCUSES ON THERMIONIC ENGINEERING, ADVANCED COMPOSITE STRUCTURES AND RESEARCH IN AIRCRAFT TECHNOLOGY; THE ARMY CURRENTLY PLANS EMPHASIS ON COMPUTER SCIENCES, VERTICAL LIFT TECHNOLOGY AND ADVANCED MATERIALS. THE NAVY HAS TARGETED ITS FELLOWSHIP SUPPORT TO STUDENTS IN ELECTRICAL ENGINEERING, COMPUTER SCIENCES, NAVAL ARCHITECTURE, APPLIED PHYSICS, MATERIALS SCIENCES AND MECHANICAL AND AEROSPACE ENGINEERING, AND IS IMPLEMENTING THEIR PROGRAM WITH THE HELP OF THE AMERICAN SOCIETY FOR ENGINEERING EDUCATION. STIPENDS OF $12,000 ARE BEING OFFERED WITH AN ADDITION OF $8,000 TO THE UNIVERSITY AT WHICH THE STUDENT DOES HIS WORK. IN THIS PROGRAM WE ARE STRIVING FOR QUALITY. THE STIPENDS ARE SUFFICIENT TO ATTRACT THE BEST U.S. STUDENTS INTO AREAS OF INTEREST TO DOD. HERE AGAIN, THIS DOES NOT SOLVE THE TOTAL PROBLEM, BUT IS A GOOD START. IT IS IMPORTANT TO EMPHASIZE THAT THESE STUDENTS ARE WORKING IN RESEARCH AREAS OF HIGH INTEREST TO DEFENSE.

OUR RESEARCH PROGRAMS INDIRECTLY SUPPORT MANY GRADUATE STUDENTS. FOR EXAMPLE, A NAVY STUDY CONDUCTED IN 1980 SHOWS THAT THE OFFICE OF NAVAL RESEARCH SUPPORTS AN ESTIMATED 2,200 GRADUATE STUDENTS (SOME PARTIALLY) THROUGH ITS CONTRACT RESEARCH PROGRAMS. THE NUMBER FOR ALL THREE SERVICES IS ESTIMATED AT MORE THAN 4,000 GRADUATE STUDENTS.

FINALLY, I WOULD LIKE TO DISCUSS OUR SMALL BUSINESS PROGRAM. UNDER THE DEFENSE SMALL BUSINESS ADVANCED TECHNOLOGY (DESAT) PROGRAM THE DOD ANNOUNCED 100 PHASE I AWARDS TO SMALL BUSINESSES ON DECEMBER 23, 1981. THESE AWARDS WERE AT THE LEVEL OF APPROXIMATELY $50 THOUSAND EACH, AND THEY WERE ISSUED FOR A SIX-

MONTH PERIOD TO SUPPORT PRELIMINARY RESEARCH AND DEVELOPMENT. THE TOTAL SUPPORT FOR PHASE I OF DESAT I WAS APPROXIMATELY $5 MILLION. BASED ON THE RESULTS OF PHASE I EFFORTS, DOD PLANS TO AWARD ADVANCED DEVELOPMENT CONTRACTS IN PHASE II, UNDER THE PLANNED DEFENSE SMALL BUSINESS INNOVATIVE DEVELOPMENT (DSBID) PROGRAM. THESE CONTRACTS WILL BE FOR FULL-SCALE RESEARCH AND DEVELOPMENT OF PROPOSED APPROACHES TO VARIOUS DOD PROBLEMS WHICH HAVE BEEN JUDGED MOST PROMISING. PHASE II AWARDS ARE CONTINGENT UPON FAVORABLE EVALUATION OF A PHASE I REPORT AND A PHASE II PROPOSAL.

VI. CONCLUSION

THERE IS A CLEAR NEED TO PLACE A HIGH PRIORITY ON THE DEPARTMENT OF DEFENSE SCIENCE AND TECHNOLOGY PROGRAM. WE HAVE ELECTED TO RELY ON OUR SUPERIOR TECHNOLOGY TO COUNTER-BALANCE THE SUPERIOR EXPENDITURES, DEPLOYMENT OF EQUIPMENT AND MANPOWER OF THE WARSAW PACT. THE BURDEN IS ON US TO MAKE THAT POLICY SUCCESSFUL. THERE IS NO DOUBT THAT THE TECHNOLOGICAL SUPERIORITY UPON WHICH WE DEPEND FOR OUR SECURITY IS BEING CHALLENGED AS NEVER BEFORE BY OUR ADVERSARIES. AND IT IS EQUALLY CLEAR THAT OUR ABILITY AS A NATION TO MEET THIS CHALLENGE WILL DEPEND IN LARGE MEASURE UPON THE MAINTENANCE OF A VIGOROUS, BROADLY-BASED, IMAGINATIVE DEFENSE PROGRAM IN SCIENCE AND TECHNOLOGY TO PROVIDE FOR CONTINUING FLOW OF NEW CONCEPTS AND TECHNOLOGICAL OPTIONS FOR THE SOLUTION OF CURRENT AND FUTURE NATIONAL DEFENSE PROBLEMS. OUR ONLY HOPE OF ACCOMPLISHING THIS TASK IS THROUGH THE CONTINUING SUPERIOR ACHIEVEMENTS RESULTING FROM OUR ACADEMIC, INDUSTRIAL AND GOVERNMENT EFFORTS IN DEFENSE RESEARCH AND DEVELOPMENT EFFORTS.

Mr. **WALGREN**. Thank you very much, Dr. Martin. I appreciate that testimony.

Dr. Langenberg.

STATEMENT OF DR. DONALD N. LANGENBERG, DEPUTY DIRECTOR, NATIONAL SCIENCE FOUNDATION

Dr. **LANGENBERG**. Mr. Chairman and members of the subcommittees, the National Science Foundation is an independent agency established by the National Science Foundation Act of 1950 with the broad mandate to promote the progress of science in the United States. The concept of a National Science Foundation emerged from the World War II recognition that a strong and broad science and technology enterprise is essential to the security of the Nation. Today it is widely accepted that this concept includes economic as well as military security. Since its establishment in 1950, NSF has occupied a unique place among Federal Government agencies, with responsibility for the overall health of science across all disciplines, in contrast to other agencies that support research in aid of specific missions such as health or defense.

The National Science Foundation maintains U.S. scientific strength by funding research in all fields of science and engineering through grants and contracts to more than 2,000 colleges and universities and other research institutions in all parts of the United States. The Foundation accounts for about 27 percent of Federal support for basic research going to academic institutions.

Given its broad mission and the range of science and engineering activities it supports, the Foundation has been vitally concerned since its inception with the subject of these hearings—setting priorities for science. Before I describe the structure that has been developed to establish priorities in NSF, I would like to offer some views on the general problem of priority-setting for science.

Science is driven by its own internal imperatives, by the rapidly evolving opportunities and advances within the conceptual structure of the disciplines. Science is also driven by the needs of the larger society for knowledge and ideas that can help illuminate and resolve important societal problems. It is the interaction, the creative tension between these internal and external imperatives that provides the dynamic context in which the problem of priority-setting for science needs to be addressed.

There are many different, equally legitimate reasons for an individual or an institution to engage in scientific research. These range from one of man's most engaging characteristics—curiosity—the desire to gain insight into a natural phenomenon without regard to any foreseeable application—to the desire to apply scientific knowledge to improve a tangible product or process or to invent a new one. In other words, the motivations of scientists and engineers and of their institutions are characterized by a large measure of pluralism.

At its best, this pluralistic nature of the science and engineering research enterprise is a kind of free competitive market of ideas, where the long-term wisdom of the whole can confidently be expected to exceed the short-term wisdom of any of its parts. This pluralism in the United States often bewilders visitors from abroad who are accustomed to working in more constrained, centrally planned settings. It is also the envy of most of those foreign visitors, who tend to agree that the array of opportunities provided by this pluralistic system is an essential component of this country's scientific strength.

Unfortunately, since opportunities for pursuing fruitful scientific research almost always exceed the financial and human resources available to pursue them, individual scientists and engineers, research institutions, and the Nation must determine what research priorities will best enable them to realize their objectives. The way a given entity determines its priorities will, of course, depend on what those objectives are, what scientific opportunities are envisioned, and what financial and human resources are likely to be available. In my view, the most important thing to remember in designing and using any mechanism for priority-setting is that it ought to provide the maximum possible flexibility at all levels, it ought to encourage the taking of risks and the seizing of opportunities, and it ought to keep options open.

Let me illustrate with an example appropriate to this season, one where success also depends on priority-setting and planning that encourage flexibility and initiative. I am thinking of the game of football—an example I owe to my colleague, Professor Raymond Orbach. Suppose some institution, a university, examines its goals and priorities and concludes that it is in its own and its patrons' interests to have a football team and to play a series of like-minded opponents. One can imagine here in the scientific and technical enterprise, perhaps the Congress and the Executive having to decide on the basis of national priorities to play the game.

This university might then hire a coach, whose responsibility it is to attract players of the highest possible individual ability and to build from them an effective team. Here one might imagine the heads of Federal research funding agencies, university presidents, department chairmen, deans, and the like. Each week during the season the coach must analyze his team's next opponent, consider the opportunities it presents, and devise a game plan that exploits his team's strengths and its opponent's weaknesses. (The analogy to the opponent of the football team in the case of the scientific and technological enterprise, I think, is ignorance.)

Saturday arrives, and the plan is put into effect. But of course everything cannot be anticipated, and the unexpected inevitably happens. For example, in the huddle, the quarterback's announced plan is to have a running back carry the ball through an anticipated hole off tackle. What if a hole develops in the center of the line while the off-tackle position is plugged by a linebacker? Should the running back follow the original plan and attempt to drive through the blocked position? Or should he seize the opportunity to run through the center instead? Will he be rewarded for following the original plan even though he is stopped cold? Will he be punished if he seizes the unexpected opportunity and makes a big gain? Should he stop in midplay and check with the quarterback or the coach before deciding what to do? If you ponder these questions, you can understand why we at NSF place a high premium on the ability to innovate within flexible guidelines, and why we try to desist from overly rigid priority-setting.

How do we at the Foundation set research priorities? We begin with NSF's Congressionally mandated mission to support long-term research. That means that we cannot establish priorities with a view toward maximizing specific economic or social returns on NSF's research investments—even though we are firmly convinced that there are substantial economic and social returns on the Nation's investments in long-term research. Rather, since NSF's primary mission is to support the best research ideas in the most important areas of science, we adhere to the principle that the scientific and engineering communities should be broadly involved in determining directions and priorities for research.

The Foundation's internal structure reflects this general principle, and the fact that since NSF is a Federal agency, it must be responsive to the external imperatives that drive science—that is, to the fact that research has important societal implications. We also believe that the mission of the Foundation clearly allows us to assist the Executive and the Congress in meeting national goals, and to help shape those goals where the contributions of science are concerned.

As a Presidential appointee, the NSF Director is responsible for assuring that Foundation priorities are consistent with broad Administration policies. As a scientist or engineer who is also a peer to the heads of other Federal research and development agencies, the Director is also in a good position to identify areas where NSF's concerns overlap with those of other agencies, and where some reasonable measure of coordination in priority-setting and program planning could be advantageous.

NSF is unique among Federal R&D agencies in having a Presidentially appointed Board—the National Science Board—which shares, with the Director, the authority and responsibility for determining the overall policies and priorities of the Foundation, including its budget priorities, within overall Administration guidelines. Since the 24-member Board—25 including the Director ex officio—is also broadly representative of the science and engineering communities, it brings to its tasks both a firsthand knowledge about the most promising areas of scientific opportunity and a sensitivity to the conditions that can best facilitate scientific research.

This extended, cooperative management structure is also reflected at the program level. The Foundation is organized into directorates, each comprised of divisions containing related groups of science or engineering disciplines. Within the organization, advisory committees associated with each division assist us in tapping the advisory resources of the scientific and engineering communities.

In short, the Foundation's institutional structure for establishing research priorities—and the detailed budget and program plans that are derived from those priorities—is intended to be responsive to the collective judgment of working scientists and engineers about where the best science is being done and is likely to be done and where the areas of greatest need are likely to be.

One indication of the sensitivity of our priority-setting mechanisms to the collective judgment of the community is the fact that changes are evident in NSF's priorities when they are viewed over a period of several years. For example, a consensus began to emerge in the mid-1970s that substantial additional investments in computer research facilities would be necessary if universities were to remain in the forefront of this important research field. The Feldstein report, completed in 1978, documented this need in detail, and recommended a program to upgrade and modernize university computer facilities.

The National Science Board accepted the essence of the Feldstein report by authorizing the Director to explore, with the Office of Management and Budget, the establishment of a new university-coordinated computer research program. This decision required a shift in the relative priorities of NSF's Directorate for Mathematical and Physical Sciences, but a shift that the university constituents of this group of programs regarded as essential. From a budget perspective, these priority changes are evident only from a perspective of several years: One grant under the coordinated program was made during fiscal year 1980, four were made during fiscal year 1981, and five in fiscal year 1982. The program continues. Ultimately, we anticipate making about 10 more grants under the program, for a total of approximately 20. The result, we think, will be a major rehabilitation of the universities ability to perform fundamental research in the computer sciences.

There are other examples. I would mention one briefly because it transcends direct NSF control and involvement: the question of obsolescent university instrumentation NSF first became aware of this problem when it was persistently identified by several of the divisions' advisory committees. When the National Science Board examined the dimensions of the problem, it agreed that support of equipment acquisition through NSF's normal grant awards process ought to be assigned a high priority. But the Board also agreed that the implications of the problem transcended NSF's interests, and that, therefore, NSF alone could not find a solution.

Because NSF enjoys good working relations with other Federal agencies, we have been able to convene, with the encouragement of the President's Science Adviser, an interagency task group to seek a coordinated Federal response. At the suggestion of the Office of Science and Technology Policy, NSF prepared a draft analysis of the problem for incorporation into the President's "Annual Science and Technology Report to the Congress, 1981" and thus helped to identify the obsolescence of research apparatus as a problem of national concern. Finally, because of NSF's many contacts in the private sector, we have been able to involve industry in the search for innovative solutions. There are other examples I could mention to show how priority setting is done in the National Science Foundation, but the central thrust of my remarks and the thought that I most want to leave with you is that since the best science is done by the best scientists and the best engineering research is done by the best engineers, these scientists and engineers also need to have a strong voice in any workable planning and priority-setting process. The Foundation system is designed to assure that they do.

I would be happy to answer any questions you may have. Thank you, Mr. Chairman.

[The statement of Dr. Langenberg follows:]

STATEMENT OF
DR. DONALD N. LANGENBERG
DEPUTY DIRECTOR, NATIONAL SCIENCE FOUNDATION
BEFORE THE
SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY
AND THE
SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT
COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

SEPTEMBER 30, 1982

Chairman Walgren, Chairman Gore and Members of the Subcommittee:

I am Donald N. Langenberg, Deputy Director of the National Science Foundation.

The National Science Foundation is an independent Federal agency established by the National Science Foundation Act of 1950 with the broad mandate to promote the progress of science in the United States. The concept of a National Science Foundation emerged from the World War II recognition that a strong, broad science and technology enterprise is essential to the security of the Nation. Today it is widely accepted that this includes economic as well as military security. Since its establishment in 1950, NSF has occupied a unique place among Federal government agencies, with responsibility for the overall health of science across all disciplines, in contrast to other agencies that support research in aid of specific missions such as health or defense.

The National Science Foundation maintains U.S. scientific strength by funding research in all fields of science and engineering through grants and contracts to more than 2,000 colleges and universities and other research institutions in all parts of the U.S. The Foundation accounts for about 27 percent of Federal support for basic research going to academic institutions.

Given its broad mission and the range of science and engineering activities it supports, the Foundation has been vitally concerned since its inception with the subject of these hearings--Setting Priorities for Science. Before I describe the structure that has been developed to establish priorities in NSF I would like to offer some views on the general problem of priority setting for science.

SCIENCE IS DRIVEN BY ITS OWN INTERNAL IMPERATIVES, BY THE RAPIDLY EVOLVING OPPORTUNITIES AND ADVANCES WITHIN THE CONCEPTUAL STRUCTURE OF THE DISCIPLINES. SCIENCE IS ALSO DRIVEN BY THE NEEDS OF THE LARGER SOCIETY FOR KNOWLEDGE AND IDEAS THAT CAN HELP ILLUMINATE AND RESOLVE IMPORTANT SOCIETAL PROBLEMS. IT IS THE INTERACTION, THE CREATIVE TENSION BETWEEN THESE INTERNAL AND EXTERNAL IMPERATIVES THAT PROVIDES THE DYNAMIC CONTEXT IN WHICH THE PROBLEM OF PRIORITY SETTING FOR SCIENCE NEEDS TO BE ADDRESSED. THERE ARE MANY DIFFERENT, EQUALLY LEGITIMATE REASONS FOR AN INDIVIDUAL OR AN INSTITUTION TO ENGAGE IN SCIENTIFIC RESEARCH. THESE RANGE FROM ONE OF MAN'S MOST ENGAGING CHARACTERISTICS--CURIOSITY--THE DESIRE TO GAIN INSIGHT INTO A NATURAL PHENOMENON WITHOUT REGARD TO ANY FORESEEABLE APPLICATION, TO THE DESIRE TO APPLY SCIENTIFIC KNOWLEDGE TO IMPROVE A TANGIBLE PRODUCT OR PROCESS OR TO INVENT A NEW ONE. IN OTHER WORDS, THE MOTIVATIONS OF SCIENTISTS AND ENGINEERS AND OF THEIR INSTITUTIONS ARE CHARACTERIZED BY A LARGE MEASURE OF PLURALISM. TWO SCIENTISTS WORKING IN THE SAME FIELD, ONE IN A UNIVERSITY DEPARTMENT AND ONE IN AN INDUSTRIAL LABORATORY MAY PURSUE A SIMILAR RESEARCH PROJECT FOR VERY DIFFERENT REASONS. TWO UNIVERSITY DEPARTMENTS WITHIN THE SAME FIELD OR TWO DIFFERENT COMPANIES WITHIN THE SAME INDUSTRY CAN HAVE VERY DIFFERENT RESEARCH OBJECTIVES AND VERY DIFFERENT RESEARCH PRIORITIES. AND, OF COURSE, SINCE THE FEDERAL GOVERNMENT SUPPORTS SCIENTIFIC RESEARCH TO FULFILL SEVERAL RELATIVELY DISTINCT MISSIONS, THE OBJECTIVES AND PRIORITIES OF DIFFERENT FEDERAL AGENCIES ALSO DIFFER CONSIDERABLY.

AT ITS BEST, THIS PLURALISTIC NATURE OF THE SCIENCE AND ENGINEERING RESEARCH ENTERPRISE IS A KIND OF FREE COMPETITIVE MARKET OF IDEAS, WHERE THE LONG-TERM WISDOM OF THE WHOLE CAN CONFIDENTLY BE EXPECTED TO EXCEED THE SHORT-TERM WISDOM OF ANY OF ITS PARTS. THIS PLURALISM OFTEN BEWILDERS VISITORS FROM ABROAD WHO ARE ACCUSTOMED TO WORKING IN MORE CONSTRAINED, CENTRALLY PLANNED SETTINGS. IT IS ALSO THE ENVY OF MOST OF THOSE FOREIGN VISITORS, WHO AGREE THAT THE ARRAY OF OPPORTUNITIES PROVIDED BY THIS PLURALISTIC SYSTEM IS AN ESSENTIAL COMPONENT OF THIS COUNTRY'S SCIENTIFIC STRENGTH.

UNFORTUNATELY, SINCE OPPORTUNITIES FOR PURSUING FRUITFUL SCIENTIFIC RESEARCH ALMOST ALWAYS EXCEED THE FINANCIAL AND HUMAN RESOURCES AVAILABLE TO PURSUE THEM, INDIVIDUAL SCIENTISTS AND ENGINEERS, RESEARCH INSTITUTIONS, AND THE NATION MUST DETERMINE WHAT RESEARCH PRIORITIES WILL BEST ENABLE THEM TO REALIZE THEIR OBJECTIVES. THE WAY A GIVEN ENTITY DETERMINES ITS PRIORITIES WILL, OF COURSE, DEPEND ON WHAT THOSE OBJECTIVES ARE, WHAT SCIENTIFIC OPPORTUNITIES ARE ENVISIONED, AND WHAT FINANCIAL AND HUMAN RESOURCES ARE LIKELY TO BE AVAILABLE. IN MY VIEW, THE MOST IMPORTANT THING TO REMEMBER IN DESIGNING AND USING ANY MECHANISM FOR PRIORITY SETTING IS THAT IT OUGHT TO PROVIDE THE MAXIMUM POSSIBLE FLEXIBILITY AT ALL LEVELS, TO ENCOURAGE THE TAKING OF RISKS AND THE SEIZING OF OPPORTUNITIES, AND TO KEEP OPTIONS OPEN.

LET ME ILLUSTRATE WITH AN EXAMPLE APPROPRIATE TO THIS SEASON, ONE WHERE SUCCESS ALSO DEPENDS ON PRIORITY SETTING AND PLANNING THAT ENCOURAGE FLEXIBILITY AND INITIATIVE. I'M THINKING OF FOOTBALL (AN EXAMPLE I OWE TO MY COLLEAGUE PROFESSOR RAYMOND ORBACH). SUPPOSE SOME INSTITUTION, A UNIVERSITY SAY, EXAMINES ITS GOALS AND PRIORITIES AND CONCLUDES THAT IT IS IN ITS OWN AND ITS PATRON'S INTERESTS TO HAVE A FOOTBALL TEAM AND TO PLAY A SERIES OF LIKE-MINDED OPPONENTS. IT HIRES A COACH, WHOSE RESPONSIBILITY IT IS TO ATTRACT PLAYERS OF THE HIGHEST POSSIBLE INDIVIDUAL ABILITY AND TO BUILD FROM THEM AN EFFECTIVE TEAM. EACH WEEK DURING THE SEASON THE COACH MUST ANALYZE HIS TEAM'S NEXT OPPONENT, CONSIDER THE OPPORTUNITIES IT PRESENTS, AND DEVISE A GAME PLAN WHICH EXPLOITS HIS TEAM'S STRENGTHS AND ITS OPPONENT'S WEAKNESSES. SATURDAY ARRIVES, AND THE PLAN IS PUT INTO EFFECT. BUT OF COURSE EVERYTHING CANNOT BE ANTICIPATED, AND THE UNEXPECTED INEVITABLY HAPPENS. FOR EXAMPLE, IN THE HUDDLE THE QUARTERBACK'S ANNOUNCED PLAN IS TO HAVE A RUNNING BACK CARRY THE BALL THROUGH AN ANTICIPATED HOLE OFF TACKLE. WHAT IF A HOLE DEVELOPS IN THE CENTER OF THE LINE INSTEAD WHILE THE OFF TACKLE POSITION IS PLUGGED BY A LINE BACKER? SHOULD THE RUNNING BACK FOLLOW THE ORIGINAL PLAN AND ATTEMPT TO DRIVE THROUGH THE BLOCKED POSITION? OR SHOULD HE SEIZE THE OPPORTUNITY TO RUN THROUGH THE CENTER INSTEAD? WILL HE BE REWARDED FOR FOLLOWING THE ORIGINAL PLAN EVEN THOUGH HE IS STOPPED COLD? WILL HE BE PUNISHED IF HE SEIZES THE UNEXPECTED OPPORTUNITY AND MAKES A BIG GAIN? SHOULD HE CHECK WITH THE QUARTERBACK OR THE COACH FIRST BEFORE DECIDING WHAT TO DO? IF YOU'LL PONDER THESE QUESTIONS I THINK YOU CAN BEGIN TO UNDERSTAND WHY WE AT NSF PLACE A HIGH PREMIUM ON THE ABILITY TO INNOVATE WITHIN FLEXIBLE GUIDELINES, AND WHY WE TRY TO DESIST FROM OVERLY RIGID PRIORITY SETTING.

HOW DO WE AT THE NATIONAL SCIENCE FOUNDATION SET RESEARCH PRIORITIES? BECAUSE OUR MISSION IS TO SUPPORT LONG-TERM RESEARCH RATHER THAN TO SUPPORT RESEARCH AIMED AT MORE SPECIFIC OBJECTIVES, WE CANNOT ESTABLISH PRIORITIES WITH A VIEW TOWARD MAXIMIZING SPECIFIC ECONOMIC OR SOCIAL RETURNS ON OUR RESEARCH INVESTMENTS-- EVEN THOUGH WE ARE FIRMLY CONVINCED THAT THERE ARE SUBSTANTIAL, BROAD ECONOMIC AND SOCIAL RETURNS ON THE NATION'S INVESTMENTS IN

LONG-TERM RESEARCH. RATHER, SINCE OUR PRIMARY MISSION IS TO SUPPORT THE BEST RESEARCH IDEAS IN THE MOST IMPORTANT AREAS OF SCIENCE WE ADHERE TO THE PRINCIPLE THAT THE SCIENTIFIC AND ENGINEERING COMMUNITIES SHOULD BE BROADLY INVOLVED IN DETERMINING DIRECTIONS AND PRIORITIES FOR RESEARCH.

OUR INTERNAL STRUCTURE REFLECTS THIS GENERAL PRINCIPLE AS WELL AS THE FACT THAT SINCE NSF IS A FEDERAL AGENCY, IT MUST BE RESPONSIVE TO THE EXTERNAL IMPERATIVES THAT DRIVE SCIENCE--THAT IS, TO THE FACT THAT RESEARCH HAS IMPORTANT SOCIETAL IMPLICATIONS. AND, IN FACT, WE BELIEVE THAT THE MISSION OF THE FOUNDATION ALLOWS US VERY CLEARLY TO HELP THE EXECUTIVE BRANCH, THE ADMINISTRATION TO MEET ITS NATIONAL GOALS, AND, IN FACT, IT HELPS SHAPE THOSE GOALS WITH RESPECT TO THE CONTRIBUTIONS THAT SCIENCE CAN MAKE. AS A PRESIDENTIAL APPOINTEE, THE NSF DIRECTOR IS RESPONSIBLE FOR ASSURING THAT FOUNDATION PRIORITIES ARE CONSISTENT WITH BROAD ADMINISTRATION POLICIES. AS A SCIENTIST OR ENGINEER WHO IS ALSO A PEER TO THE HEADS OF OTHER FEDERAL R&D AGENCIES, THE DIRECTOR IS ALSO IN A GOOD POSITION TO IDENTIFY AREAS WHERE NSF'S CONCERNS OVERLAP WITH THOSE OF OTHER AGENCIES AND WHERE SOME REASONABLE MEASURE OF COORDINATION IN PRIORITY SETTING AND PROGRAMING PLANNING COULD BE ADVANTAGEOUS.

IN ADDITION, NSF IS UNIQUE AMONG FEDERAL R&D AGENCIES IN HAVING A PRESIDENTIALLY APPOINTED BOARD -- THE NATIONAL SCIENCE BOARD --WHICH SHARES, WITH THE DIRECTOR, THE AUTHORITY AND RESPONSIBILITY FOR DETERMINING THE OVERALL POLICIES AND PRIORITIES OF THE FOUNDATION, INCLUDING ITS BUDGET PRIORITIES, WITHIN OVERALL ADMINISTRATION GUIDELINES. SINCE THE 24 - MEMBER NATIONAL SCIENCE BOARD IS ALSO BROADLY REPRESENTATIVES OF THE SCIENCE AND ENGINEERING COMMUNITIES, IT BRINGS TO ITS TASKS BOTH A FIRST HAND KNOWLEDGE ABOUT THE MOST PROMISING AREAS OF SCIENTIFIC OPPORTUNITY, AND A SENSITIVITY TO THE CONDITIONS THAT CAN BEST FACILITATE SCIENTIFIC RESEARCH.

THIS EXTENDED, COOPERATIVE MANAGEMENT STRUCTURE IS ALSO REFLECTED AT THE PROGRAM LEVEL. THE FOUNDATION IS ORGANIZED INTO DIRECTORATES, EACH COMPRISED OF DIVISIONS WHICH ARE IN TURN COMPRISED OF RELATED GROUPS OF SCIENCE OR ENGINEERING SUBDISCIPLINES. EACH DIVISION IS HEADED BY A SCIENTIST OR ENGINEER WHO IS ULTIMATELY RESPONSIBLE, THROUGH THE DIRECTORATE, TO THE NSF DIRECTOR. PRIORITIES WITHIN EACH DIVISION ARE ESTABLISHED BY THE DIVISION IN CONSULTATION WITH AN ADVISORY COMMITTEE OF WORKING SCIENTISTS OR ENGINEERS WHO ARE REPRESENTATIVE OF THEIR RESPECTIVE FIELDS.

In short, the Foundation's institutional structure for establishing research priorities -- and the detailed budget and program plans that are derived from those priorities -- is intended to be responsive to the collective judgment of working scientists and engineers about where the best science is being done and is likely to be done and where the area of greatest need are likely to be.

One indication of the sensitivity of our priority setting mechanisms to the collective judgment of the community is the fact that changes are evident in NSF's priorities when they are viewed over a period of several years. For example, a consensus began to emerge in the mid-1970s that substantial additional investments in computer research facilities would be essential if universities were to remain in the forefront of this important research field. The Feldstein report, completed in 1978, documented this need in detail, and recommended a program to upgrade and modernize university computer facilities. The National Science Board accepted the essence of the Feldstein report by authorizing the Director to explore, with OMB, the establishment of a new university coordinated computer research program. This decision required a shift in the priorities of the Directorate of Mathematical and Physical Sciences, but a shift that the university constituents of this group of programs regarded as essential. From a budget perspective, these priority changes are only evident from a perspective of several years: one grant under the coordinated program was made during FY 1980, four were during FY 1981 and five in FY 1982. Ultimately, we anticipate making about ten more grants under the program, for a total of approximately 20.

The NSF decision and priority setting system has also shown itself to be effective in identifying high priority problems that transcend NSF's mission, and also in seeking solutions that involve other institutions besides NSF. Consider, for example, one of the major problems that presently confronts the U.S. scientific community: the critical and growing obsolescence of research apparatus in our university laboratories. The seriousness of this situation is now recognized as a problem not only for the universities, which often cannot conduct adequate research programs for want of appropriate apparatus, but also for industry and Government laboratories whose own research programs rely on a steady flow of innovative results from the universities.

NSF first began to be aware that the obsolescence of research apparatus was -- and is -- a problem cutting across a wide range of fields when it was persistently identified as such by several of

OUR DIVISIONAL ADVISORY COMMITTEES WHICH ARE COMPOSED, AS YOU WILL RECALL, OF WORKING SCIENTISTS AND ENGINEERS. WHEN THE NATIONAL SCIENCE BOARD EXAMINED THE DIMENSIONS OF THE PROBLEM IT AGREED THAT SUPPORT OF EQUIPMENT ACQUISITION THROUGH NSF'S NORMAL GRANT AWARDS PROCESS OUGHT TO BE ASSIGNED A HIGH PRIORITY. BUT THE BOARD ALSO AGREED THAT THE IMPLICATIONS OF THE PROBLEM TRANSCENDED NSF'S INTERESTS, AND THAT THEREFORE NSF ALONE COULD NOT FIND A SOLUTION. BECAUSE NSF ENJOY'S GOOD WORKING RELATIONS WITH OTHER FEDERAL AGENCIES, WE HAVE BEEN ABLE TO CONVENE, WITH THE ENCOURAGEMENT OF THE PRESIDENT'S SCIENCE ADVISER, AN INTERAGENCY TASK GROUP TO SEEK A COORDINATED FEDERAL RESPONSE. AT THE SUGGESTION OF OSTP, NSF PREPARED A DRAFT ANALYSIS OF THE PROBLEM FOR INCORPORATION INTO THE PRESIDENT'S ANNUAL SCIENCE AND TECHNOLOGY REPORT TO THE CONGRESS, 1981 AND THUS, HELPED TO IDENTIFY THE OBSOLESCENCE RESEARCH APPARATUS AS A PROBLEM OF NATION CONCERN. FINALLY, BECAUSE OF NSF'S MANY CONTACTS IN THE PRIVATE SECTOR, WE HAVE BEEN ABLE TO INVOLVE INDUSTRY IN THE SEARCH FOR INNOVATIVE SOLUTIONS.

I HAVE NOT DESCRIBED THE EQUIPMENT OBSOLESCENCE PROBLEM TO TRY TO CONVINCE YOU THAT THE PROBLEM IS, IN FACT, CRITICAL. RATHER, I HAVE USED IT TO ILLUSTRATE HOW NSF'S STRUCTURE AND FUNCTIONS IDENTIFIED AND DELINEATED A PRIORITY ISSUE OF SIGNIFICANCE BOTH TO NSF AND OTHERS, AND HOW, BECAUSE OF OUR STRUCTURE AND FUNCTIONS, WE HAVE BEEN ABLE TO BRING OTHER PARTIES INTO THE SEARCH FOR SOLUTIONS. I COULD JUST AS EASILY HAVE CHOSEN OTHER EXAMPLES SUCH AS THE SHORTAGE OF ENGINEERING SCHOOL FACULTY OR THE DETERIORATING CONDITION OF PRECOLLEGE MATHEMATICS AND SCIENCE EDUCATION TO ILLUSTRATE SOME OF THE MORE GENERAL REMARKS I MADE EARLIER IN MY TESTIMONY. THE CENTRAL THRUST OF THOSE REMARKS, AND THE THOUGHT I WANT TO LEAVE YOU WITH, IS THAT SINCE THE BEST SCIENCE IS DONE BY THE BEST SCIENTISTS, THE BEST SCIENTIST ALSO NEED TO HAVE A STRONG VOICE IN ANY WORKABLE PLANNING AND PRIORITY SETTING PROCESS.

I'LL BE HAPPY TO TRY TO ANSWER ANY QUESTIONS YOU MAY HAVE.

Mr. BROWN. Thank you, Dr. Langenberg.

The three of you have given us an excellent background in processes for priority setting. In your various agencies you represent a very large majority of the Federal involvement in support of science, and we appreciate your statements very much.

I will call on Mr. Gore first for any questions.

Mr. GORE. Thank you very much, Mr. Chairman.

I would like to apologize to the witnesses for being a bit late at the beginning and also for the fact that I am going to have to testify before the Rules Committee and I have an amendment on the floor. This has turned out to be the last week of the session. It wasn't scheduled to be that, but as a result, a lot of things are running into each other on the way to the floor and it is going to be a busy time for us, and so I apologize for the fact that I will not be able to be here for the entire session.

I thought the three statements were excellent, and I noticed, Dr. Martin, the aplomb with which you delivered your statement. The last time you were here, if I am not mistaken, it was the first time you had ever testified. That seems like 15 or 20 years ago.

Dr. MARTIN. To you.

Mr. GORE. Such a smooth delivery. I appreciated the statements very much. I am sorry that OMB decided that it couldn't come, because it is really making a lot of the important decisions and the setting of priorities on research and development depends in a large part on what OMB does, and they just decided at the last minute that they didn't want to talk about this because there were other things going on, or whatever.

We found out about it late last night. I think some of their approaches are unfortunate. The statement that we can't any longer aspire to be the leader in these fields is really kind of depressing to me, and I think something that ought to be challenged in the scientific community, and I hope will be challenged here in the Congress.

Let me ask a slightly different question and invite the three of you to respond briefly, if you would. Are there cases in which the Federal Government should finance applied or development research with commercial applications? How do you look at a question like that? Whoever wants to go first.

Dr. WYNGAARDEN. I believe the answer to that is yes; there are many examples that would illustrate this. For example, during the past 15 to 20 years, something on the order of 1,000 patents have been obtained on NIH supported work and 49 percent of those have actually been licensed; thus, a very heavy emphasis in biomedical science is on developing products, procedures, and instruments that will advance health care.

The commercialization of those developments is something to which we pay particular attention. In fact, we have a division within the Director's office called the Office for Medical Applications of Research which has that as a major responsibility—to make certain that the lag between discovery and application is minimized and that those developments are brought to the attention of the public as rapidly as possible. We have also some extensive patent arrangements with universities—that you are acquainted with, I know—that give them the first right to develop and license the patent and the Government marketing rights if they fail to do so.

Dr. MARTIN. I think as NSF had indicated, it is not a simple separation of church and State and that the economic health of the country is a factor in the security of the country as well.

I think that we are also seeing in other nations a coupling of their efforts between government and industry and defense and industry. We may take heed because this coupling is very effective in expediting the transition of the 6.1 basic research concepts into realization.

For us in defense, our strategy has as its cornerstone high technology and we have to acknowledge that the world climate today is one in which there is both an intellectual capability and an industrial base existing with our adversaries to compete with us and to take our research results and put them into real systems very, very expeditiously. We can't sit back and wait for the process to take place as it did 10 years ago.

So I think a fostering of that would be beneficial to the defense mission.

Dr. LANGENBERG. The Foundation's mission encompasses support of research but not of development. However, within that mission, NFS has taken a number of steps to assure that the research knowledge gained from the research that NSF supports finds its way to such applications and such development operations as may be justified.

For example, NSF's industry-university cooperative research program supports research performed is cooperatively by researchers in industry and in universities. The idea is that the industry is the appropriate mechanism to carry forward any new knowledge gained from that research through the development process to a commercial product.

NSF began in 1977 a small business innovation research program. There again, the foundation supports research in small businesses, not development, but research with the clear intent that if successful, it may lead to commercial developments.

Within the past several years, NSF purposely as a matter of policy has eliminated any specific references to basic or applied research within the Foundation's programs. Any serious examination of how basic research may be distinguished from applied research leads into a quagmire; it is difficult to do in a way that is truly functional. Therefore, we have concluded that we should not do it. NSF will support research, it will concentrate on excellence, and the balance between basic and applied research will be determined primarily by the dynamics of the engineering and scientific disciplines.

Mr. GORE. I note that there seems to be an abundance of commonsense in the testimony of the witnesses and express regret that that commonsense isn't reflected always at OMB and actually at the White House there have been some statements about a sharp dividing line and anything that might possibly dissuade our quintessential hypothetical entrepreneur from getting involved in a research area, certainly the Government should bend over background not to even get involved.

That is what the White House seems to be saying; but it is an unrealistic position because in most cases the net result is that it won't get done at all and we are facing a challenge in the world where we have to do more research and we have to recognize that the matching of the national economy with the accelertaing scientific revolution means that national advantages in a variety of fields will come from better research and more research, and if we don't do it, we are not helping ourselves at all. We are hurting ourselves.

Mr. GORE. I feel I have got to go to the Rules Committee. Before I do, Mr. Chairman, I would like to issue a special welcome to my fellow Tennessian, Dr. Alvin Weinberg, who is as well thought of in Tennessee as he is in the rest of the country with it's national reputation in this and related fields. We are glad to have you with us.

Mr. **Brown**. I always thought Dr. Weinberg was a Californian who was temporarily——

Mr. **Gore**. No, he likes it there and we have him.

Mr. **Brown**. Mr. Skeen, would you like to have a few questions or introductory remarks in defense of OMB?

Mr. **Skeen**. That is a wonderful opportunity you have left me!

It has been alleged that cutbacks in federal funding for civilian science will allow only the most prestigious and elite universities in the Nation to continue to do research and such a trend would be contrary to the congressionally enunciated policy that funds should be fairly distributed.

Which areas would suffer the most in the cutbacks of civilian funding to universities and what can be done to make sure that the science resources are distributed equitably throughout the Nation?

Dr. **Langenberg**. I am not entirely sure that the assumption that there are cutbacks in the funding of civilian science is supportable in fact. This administration has supported research strongly. That does not mean that the budget for research and development, like the budget of the Federal Government, is not under considerable strain. It is.

But I really do not expect to see any major change in the distribution of federally supported research among areas of the country except such changes as may occur from the rise and fall of scientific capability in individual institutions and among individual researchers.

Mr. **Skeen**. I think that is an interesting response, Doctor, because the perception is, and among the members of the panel, that there is a cutback. I am talking about the congressional side, because that is the basis from which we launch almost every argument that we have about reducing our commitments to the scientific community and so forth, that it is a cutback.

I also appreciate the quality of your response because I think it gets to the heart of what the real problem is and what we are really trying to do.

Would any other panel members like to contribute?

Dr. **Martin**. Your question was addressed to civilian science and therefore the Department of Defense chooses not to respond.

Mr. **Skeen**. As you know, we have Los Alamos and Sandia Labs in New Mexico and there is an article this morning about the difficulty Los Alamos is having trying to convert itself to civilian scientific interests. I just scanned the article before I came here, and I think that is another misconception.

Los Alamos, and most of the national laboratories have been involved not only in DOD projects but also have a strong commitment to civilian projects and those scientific efforts that have civilian application. It is important that we highlight that.

Dr. **Martin**. There can be little doubt that there is tremendous civilian fallout to research that has been done in the DOD. Integrated circuits are a fine example.

Mr. **Skeen**. Absolutely.

Dr. **Wyngaarden**. I agree with Dr. Langenberg. The NIH program is a broad one, distributed throughout the country. We support over 18,000 projects altogether—14,000 principal investigators found in over 1200 institutions, of which over 600 are considered research-intensive institutions. These involve every State and every corner of the union, so I don't think there has been any cutback in the sense of geographical distribution.

Mr. SKEEN. This will be the final question: In a recently published article, Dr. Keyworth said, among other things, that the Government had very little role in developing commercially viable technology. Dr. Martin, do you think that the aerospace companies that build our military aircraft and other weapon systems would agree with that assessment, and isn't the research that underpins the development of hardware in large measure supported by Federal funding?

Dr. MARTIN. I would rather you asked NASA that. Certainly if we look at the contract dollars that are let, approximately 3 percent of the contract dollars that are let to industry are available to industry to do internal research and development. So I think we are being very supportive of industry, and we believe in the case of the Department of Defense that that is going to have some military application. We realize that it is a seed that is placed in very fertile ground and it may have commercial implications.

Our mission is defense and certainly we want to see the dollars invested apply to that purpose. We fully recognize that there will be other benefits, and as I mentioned earlier, the economic health of the country is also beneficial to the Department of Defense.

As far as the aircraft industry is concerned, I don't think anyone can deny that a good percent of that, in the past, has been funded in part by government agencies. I think that if you would participate in some of the National Academy of Science committees that have been taking place over recent months, you would find that the pullback in that direction is of great concern to the industry. They feel that the underpinnings of their past successes are falling apart and that the future in both general aviation and in military aviation can be quite dreary for U.S. industry.

Again, back to your earlier statement; is there a need for interdependence. Personally, and for the Department, I believe that there is.

Mr. SKEEN. Thank you very much, Dr. Martin.

Mr. BROWN. Dr. Langenberg, you in your statement gave some interesting examples of the long term priority setting process in NSF. Focusing on examples like the growing awareness of the need for improved instrumentation in universities and the importance of I guess a subset of that problem, a need for better computing capabilities.

The question that I would have is whether or not this shift has—this recognition has actually been adequately implemented in terms of the overall governmental response to that need. In other words, you have a perception of a high priority need.

Obviously, we had some trouble in funding that in the NSF budget. You have indicated where you have tried to get the entire Federal structure to recognize and act on it. Do you think the problem has been adequately addressed in the responses that have been made to date, including what each of the agencies represented here said?

Dr. LANGENBERG. I think we are just beginning to address that problem. It took us a long time to create it, and I think it will take quite a while to dig ourselves out of the hole that we have made for ourselves.

There are a number of things that we think need to be done to help get ourselves out of that hole, and only one of them is a direct budgetary response by the Federal Government through its R&D funding agencies to provide funding for instrumentation.

With discussion and debate in the interagency working group, all of the agencies have begun to look at other ways of dealing with the problem. Let me mention a few.

We need better information about the status of scientific research instrumentation in the universities. You are familiar, Mr. Brown, with the National Science Board report on indicators of the status of science and technology in this country in comparison with nations abroad. We don't have a good indication, and we have embarked on a project to see if we can develop one and use it in a continuing way so that we can tell what is happening.

Clearly, the universities themselves have a large stake in the solution to this problem, and we believe that there are steps that they can take to help solve it. For example, the tradition in university research is that the way to fund significant capital investments and equipment is to go to an outside source—perhaps a private donor, perhaps the Federal Government—get what amounts to a check, and attach a purchase order.

Many people who finance research and development and capital acquisitions in industry are horrified by that procedure. No industry management in its right mind would conceive of doing something like that. Rather it would borrow, it would lease, it would do other things.

It seems to us that the universities perhaps need to think more creatively and innovatively about how to fund, in the long term, significant capital investments in research administration as they now creatively finance new classroom buildings and new dormitories.

We also think there is a lot of room for improvement in the way the private sector responds to this problem. I mentioned that NSF has had some luck in involving the private sector in working with the Foundation. For example, a recently completed set of grants supports researchers who will develop software for the teaching of mathematics, science and engineering. That program did not involve provision of funds for any of the computer equipment that would be required to implement that software. Those funds came from the private sector by donation to NSF's grantees. And the funding was coordinated with the Foundation.

There are significant incentives for donations and for support of university research in the Economic Recovery Tax Act of 1981. We think more needs to be done to encourage industry and universities to make use of those incentives. The awareness of the benefits of their use is only now becoming apparent to the universities and to industry.

The impression I am trying to create is that there are many aspects to this problem, but only one of them, an important one, is direct funding to solve the university research instrumentation problem.

Mr. **Brown**. Dr. Langenberg, I hope you will continue with your efforts to develop suitable indexes of status of funding. As you have described the growth of the problem, it seemed to gradually seep into the consciousness of the policy community as a result of intangibles I guess you might say and was recognized and action was taken, but I think we need a more systematic process for addressing major problems.

As long as we are on that, Dr. Martin, you described the DOD program. As I recall, you indicated that there is a 5-year program calling for the development of $30 million per year?

Dr. **Martin**. Correct.

Mr. **Brown**. And I want to ask you, what is the health and status of that program? Are you running into any roadblocks either in the executive or the Congress with regard to continuing the funding of that program?

Dr. **Martin**. I am going to call on Leo Young, who is one of my directors and who has been directly in charge of that program.

Dr. **Young**. We have been receiving accolades for the program from both the Senate and the House.

Mr. **Brown**. It is not accolades you need, you need money.

Dr. **Young**. I am coming to that. The problem is that we had anticipated growth in the basic research program over and above the instrumentation program, and unfortunately some committees in Congress have been looking at the bottom line only, and so having started the instrumentation program, and we are proud of it, the situation now is that it may result in a reduced effort in the research itself, and this concerns us very much. It is money we need, not accolades.

Mr. **Brown**. You mean that the appropriate committees are taking money out of the research itself in order to continue the instrumentation program?

Dr. **Young**. No. We have what we call program elements, the total program element is what is visible. The instrumentation we intend to maintain regardless of what happens, but the instrumentation does not pay for research as such, and so when you allow for inflation in the instrumentation program, what is left may well be less than the research effort we had last year.

Mr. **Brown**. I have a larger problem that I would like to address to you in connection with the overall problems of scientific priorities. We have a situation where we are faced with national competition, or competition between whole societies, not only in the military but in the supporting scientific and technological base. Some of these societies seem to have invented devices which for example channel a much larger proportion of their society into scientific, engineering, and technological work. The Japanese are an example that is frequently cited, the large number of engineers compared to lawyers, for example.

Suppose it gets down to the point where any reasonable analyst would hold that the progress that each of these societies makes is really a function of how much intelligent manpower they devote to it, and we are lagging in that race. Where in the priority-setting process do we recognize this and try to do something about it? You have faced that in health. We have a system which obviously has provided us with the largest number of trained health personnel in the world, probably, on a per capita basis.

Dr. **WYNGAARDEN**. It is a very real problem in the health field just now because the interest of young people, particularly medically trained people, in biomedical research has sagged in the past 10 or 15 years. There has been some recovery in the last few years, but we are facing a growing shortage of trained scientists who have a medical background. Fortunately, the supply of scientists with Ph. D. training has held up very well.

One of the responses on the part of NIH has been to increase the incentives for medically trained manpower in this field. Another has been the policy of stabilization, because a good deal of the attraction for medically trained people to biomedical science depends on the stability of the system. If they sense that there is opportunity for new research grants for the young people coming in, and that there is a reasonable opportunity for continued funding, then this field remains attractive to them.

For that reason we have put primary emphasis on the investigator-initiated research grant as a chief vehicle for research, and have made cuts in other program areas in order to maintain the number of such grants at as high a level as we can. The President's budget for fiscal year 1983 would allow us to fund 4,100 such new starts. When the economy improves, we hope that we can get closer to the range of 5,000, which is sort of an average of the last several years, and which has assumed some significance as a target figure. We believe that this is still a valid figure for the future and that we are coming as close as we can at present. But the effort to stabilize new and competing awards within our various mechanisms has in turn carried that message to the young scientist and has had some effect both in reducing the downward slide of interest and in seeing it return over the last 2 or 3 years to a modest extent.

Mr. **BROWN**. Is not your basic problem though in getting medically trained researchers the fact that you cannot be competitive with the remuneration offered in private practice?

Dr. **WYNGAARDEN**. That is one of the problems. There are many others. I think there remains a good percentage of highly motivated people who are fascinated by the creative opportunities of research and are not asking for the same income as they might achieve in practice. I think what they need, however, is a reasonable assurance that the system is stable.

Mr. **BROWN**. Well, that is only a part of the problem that I am trying to raise here. Dr. Langenberg, we have discussed this before, but if my assumption—and I am just offering it as a hypothesis—that ultimately the health and competitiveness of our society depends upon the degree to which we attract scientific and technical manpower into careers, and we are suffering in that regard, does this come into your calculus as a priority within science at some point, or does it not? And to be specific, if it requires, for example, the enhancement of scientific education in the elementary and high schools or improvement of teacher training or other things of that sort, how do we recognize this?

Dr. **LANGENBERG**. As you know, Mr. Brown, this set of questions has a very high priority with the National Science Foundation and with the National Science Board. I know of no other set of questions that the Foundation staff and the members of the Board have spent so much time on in the last couple of years. Let me make several comments.

First, I believe that right now, in most fields of science in which the National Science Foundation supports research, we are more money-limited than we are people-limited. The evidence exists among the 27,000 proposals NSF receives every year; more than half of those proposals NSF is unable to fund, and NSF simply has to decline many excellent proposals from many first-rate scientists.

The statement that we are more money-limited than people-limited may not be true in all fields. There are serious reasons for concern now about university research capabilities in certain fields such as engineering disciplines and computer sciences and certainly a large part of the reason is the strong competition with demands of the private sector for people with skills in these areas—skills that the private sector is willing to acquire at prices, or if you like, salaries substantially higher than universities are able to offer.

In the long run, I think you are absolutely correct that the most critical aspect to the Foundation for the scientific and technological future is certainly the people who are going to create that future. Of course, there are serious reasons for concern. It is becoming increasingly evident to more and more of the population that there are serious deficiencies in scientific, technological, and mathematical education at all levels, but most particularly at the precollege levels. If I remember correctly, about half of all the high school mathematics teachers in this country are formally unqualified to teach what they are teaching, and the situation is getting worse.

As you know, the National Science Board has formed the National Science Board Commission on Precollege Education in Mathematics, Science, and Technology. The Commission is trying to provide some leadership, trying to provide a national agenda, an action plan through which we all can address this problem. It is assuredly not solely a problem for the Federal Government. The education system of this country is an enterprise which runs at a level of between $150 billion and $200 billion a year, and the Federal Government has never contributed more than a small part of that total investment.

Looking perhaps a little farther ahead, one can see the demographic problems that have been for the universities sources of concern for the scientific and technological enterprises. There will be fewer young people through the remainder of the 1980's and the 1990's than we have been accustomed to, and therefore fewer young scientists and engineers, at a time when we are going to need all the scientific and engineering know-how that we can acquire.

Fortunately, there is one large possibility for solving that personpower problem. It is a fact that we have been underexploiting about two-thirds of the potential pool of young people as possible sources of tomorrow's scientists and engineers. That two-thirds happen to be women and/or minorities. They represent an opportunity for at least part of a way out of the demographic dilemma. But there again, everything depends upon early schooling. If the system provides inadequate education in mathematics and in science, if for whatever reason it discourages any part of the population from acquiring that crucial start in the language of science and mathematics, then significant damage may result. It is a large and complex problem, one which seriously concerns the National Science Foundation.

Mr. BROWN. I appreciate the response. I am not trying by the question to lead to the assumption that there is a Federal solution to problems of that magnitude. I think we are no longer that naive. But we are talking here this morning about priorities, and there has to be some way of having generated a concept of priorities that involve the whole Nation. As you are aware to some degree the problem of instrumentation was an example of this, but we are talking about even broader problems here when we talk about education or perhaps the supply of medically trained researchers or the balance between civilian scientists and military-oriented scientists. Somewhere before it becomes a crisis, problems developing in these areas have to be identified, and the attention of the Nation focused on them, so that at each level, whether the local school systems or whatever, proper attention can be given to them.

I could pursue this interesting discussion at much greater length, but because we have a number of other witnesses, I am going to refrain. Thank you for your excellent contribution this morning. If we want to badger you further, we will submit some questions in writing and hope that you will respond to them. Thank you very much.

Mr. GORE. Our next panel consists of Dr. Weinberg, director of the Institute for Energy Analysis at Oak Ridge Associated Universities; Dr. Simon Rottenberg, professor of economics at the University of Massachusetts, and Dr. Richard J. Hill, who is provost and vice president for academic affairs at the Univeresity of Oregon.

Mr. BROWN. Gentlemen, we are pleased that you are here. I presume that you are in the class that we can describe as the elder statesmen of science, or is that just you, Dr. Weinberg? At least the seers of the future.

I am glad that you were all here to listen to the testimony of the witnesses representing the various Federal agencies, and perhaps that can assist you in directing some of your own comments on this subject.

As the chairman indicated earlier, the full text of each of your statements will be inserted in the record and you may present it in such fashion as is most convenient for you, and we will start in the order that I called you, Dr. Weinberg, and you may proceed.

[The biographical sketch of Dr. Weinberg follows:]

DR. ALVIN M. WEINBERG

Alvin M. Weinberg is director of the Institute for Energy Analysis, which he was instumental in establishing at Oak Ridge Assocaited Universities in January 1974.

Weinberg joined the Oak Ridge National Laboratory (ORNL) in 1945 as one of the first members of the University of Chicago's wartime Metallurgical Laboratory. He served as Director of the Physics Division (1947-1948), as Research Director (1948-1955), and Director (1955-1973). In 1974 he was Director of the Office of Energy Research and Development, Executive Office of the President.

He was the originator of the pressurized water reactor and proposed its use for submarine propulsion in 1944.

As a writer and lecturer on science policy, Weinberg has contributed to the formulation of public policy concerning the relationship between science and government. He has continued to articulate the issues associated with energy policy and with the relation between technology and society; and he has written and lectured extensively, both in the United States and abroad, on some of the difficult problems of public policy posed by the growth of modern science. He also written on the criteria for scientific choice—that is, the criteria for deciding which fields of science deserve the most public support in a situation where public support is limited.

Weinberg received the S.B., S.M., and Ph. D. in physics at the University of Chicago.

For his role in the development of nuclear reactors, Weinberg shared the Atoms for Peace Award in 1960 and was one of the first recipients of the E. O. Lawrence Memorial Award. In 1966 he received the University of Chicago Alumni Medal; in 1971 he received the American Nuclear Society Chernick Memorial Award; and in 1975 he was awarded the first Heinrich Hertz Prize on the 150th anniversary of the University of Karlsruhe. He was the recipient of the New York Academy of Sciences Award for 1976; the Fermi Award of the Department of Energy in 1981; and the Harvey Prize of the Technion, Israel Institute of Technology in 1982. Weinberg is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the National Academy of Engineering, and the Royal Netherlands Academy of Arts and Sciences.

STATEMENTS OF DR. ALVIN M. WEINBERG, DIRECTOR, INSTITUTE FOR ENERGY STUDIES, OAK RIDGE ASSOCIATED UNIVERSITIES, OAK RIDGE, TENN.

Dr. WEINBERG. Thank you very much, Mr. Chairman.

I am pleased and flattered to have this opportunity to give my views on priorities in science.

I should make clear that although I participated in the debate in the 1960's, 20 years ago, I have been mostly an interested spectator in recent years. I will try to give some historical perspective and perhaps a few remarks on what I perceive to be the present situation.

The paper by Bruce Smith, which was distributed to the committee, gave a very good statement of where the debate on allocation for science stood in the mid-1960's, and I guess I have a feeling of deja vu.

The issues that trouble science policymakers today and the Congress: How much should be allocated to science in comparison to other activities of Government? How should resources allocated to science be divided among different scientific fields? These questions remain much the same as those debated 20 years ago.

This gives us a sense of humility that we can't expect to get revealed wisdom on the issue of priorities in science. Each generation will address essentially the same questions, but always with a concern for practical exigencies of the time. Today those are basically the strong reduction in nondefense elements in the Federal budget and an even stronger increase in the defense budget.

I became involved in the debate on priorities when I was a member of the President's Science Advisory Committee from 1959 to 1962. At that time the central question of science policy was whether or not the United States should go to the Moon. President Eisenhower had decided against sending a man to the Moon, but that decision was reversed by President Kennedy. This decision brought into strong focus the whole conceptual underpinning for making choices in science. At the same time, Prof. Ed Shils, a distinguished sociologist at the University of Chicago, founded the journal "Minerva"; one of its purposes was to provide a forum for the scholarly debate over how to set priorities in science.

The first article of "Minerva," issued in autumn 1962, was entitled "The Republic of Science, Its Political and Economic Theory," by Michael Polanyi. In this article, which had tremendous influence on much of the thinking about scientific priorities, Polanyi likened the collective action of all scientists, to an interacting economic and political republic governed, in analogy with ordinary economic activity, by what Harvy Brooks calls the intellectual marketplace.

In Polanyi's view, because of the intrinsic impossibility of foreseeing where science is going, the invisible hand, operating by countless competing scientists and scientific doctrines, provides the best way of allocating resources. Good science and good scientists inevitably push out bad science and bad scientists. In short, the free market works far better for allocations in science than does any form of socialism: Polanyi's position may almost be described as a reduction of science policy to a branch of economics, with the intellectual marketplace doing the allocating.

I was interested in Dr. Langenberg's testimony in which he again stressed, in fact each witness stressed the best way to allocate the money is to find out who the best scientists are and they will do the allocating much better than any bureaucrat or even, Mr. Brown, Congress.

Mr. **Brown**. Oh, no, not that.

Dr. **Weinberg**. My first article, "Criteria for Scientific Choice," conceded that Polanyi's Republic of Science worked tolerably well for little science, that is, science done in more or less a traditional way by individuals or small groups whose demands on the budget were not very large. But for the really large decisions—whether to go to the Moon or build a 1000-GeV accelerator or a space telescope—I suggested that criteria internal to science and derived from the intrinsic logic of the science were insufficient. For such decisions, criteria external to science, or to a given field of science, were necessary.

I described these as technological merit, social merit, and scientific merit. In each instance, the value of the proposed science is measured by its effectiveness in achieving an end that arose from outside the science, say a technological aim, like fusion energy, or a social aim, like military strength or, in the case of scientific merit, progress in neighboring fields of science.

My criteria of choice amounted to setting up a system of scientific values. Philosophers of science have always been concerned with scientific truth: they wrestle with the question of how to test the truth of a scientific finding. I was asking a different question of two sciences that were equally true, how do you decide which one is more valuable than the other?

I used to say that this question gave the philosophic underpinning not for science itself, which was the realm of the philosophy of science, but rather for the field of scientific administration, because scientific administrators have to make the choice between different competing scientific efforts that to decide which is the more valuable, and we were trying to give a scientific underpinning for such decisions.

In the intervening years, several attempts have been made to apply these scholarly speculations to actual allocations in science. Perhaps most notable were a series of reports put out by the National Academy of Sciences in which broad fields of science were assessed. The most ambitious report was the one on physics carried out under the chairmanship of Pro. Allen Bromley of Yale University in 1972.

The Bromley committee tried to use explicit criteria, largely based on those I had delineated, to rank the various subfields of physics. Their top three subfields and institutions of physics—lasers and masers, quantum optics, and elementary particles—today continue to receive very strong support. On the other hand, their next highest ranking field, nuclear physics, no longer seems to receive as much support as the Bromley committee recommended.

The zenith of this kind of discussion I think occurred at the time of the study chaired by George Kistiakowsky, the former President's science adviser, President Eisenhower's science adviser. He undertook a study sponsored by a predecessor of this congressional committee; the study was called basic research and national goals.

I went to my library amd dusted it off and there it is. It appeared 17 years ago. It consisted of 15 separate essays by members of the panel. The entire panel criticized each essay, though each essay was the responsibility of its author and each author was allowed to accept or reject the criticism.

The summary was drafted by me, but with very strong input from George Kistiakowsky, so it was sort of a combination effort between him and me. As one would expect, basic research and national goals covered the whole range of opinions as to how much science ought to be supported—from a high of 10 to 15 percent growth per year to this delightful, almost tongue-in-cheek remark by the economist Harry Johnson—unfortunately he is deceased now. Let me quote his words, though I hope you don't take them too seriously, Chairman Brown.

> The argument that individuals with a talent for such research should be supported by society . . . differs little from arguments formerly advanced in support of the owners of landed property to a leisured existence . . . insistence on the obligation of society to support the pursuit of scientific knowledge for its own sake differs little from the historically earlier insistence on the obligation of society to support the pursuit of religious truth, an obligation recompensed by a similar unspecified and problematical payoff in the distant future.

I hope the committee doesn't take that too seriously, although I feel my colleague Dr. Rottenberg has been a bit too influenced by his distinguished colleague in economics, Professor Johnson.

My own views began with a rejection of science as a whole as a valid category. Instead, science that was clearly relevant to achievement of an applied aim ought to be viewed as an overhead, assessed not against some fictitious scientific budget, but rather against the budget for achievement of that end; for example, research on lasers relevant to design of antitank weapons ought to be charged to antitank defense, not to science.

By implication this tends to answer the question earlier whether the Government ought to be in applied science. Thus every agency of Government ought to allocate some of its budget to the use of science in the achievement of its aim, just as every corporation allocates some of its budget to scientific research because it believes that research will pay off in better products or services. What that fraction allocated to research is cannot be judged a priority—and, to be sure, it will fluctuate as the priority for the underlying nonscientific aim fluctuates.

In addition to the science more or less directly tied to the achievement of nonscientific aims, and therefore properly supported by Government agencies such as Agriculture or Interior or Defense—that regard science simply as a means to an end, not an end in itself—most of our panelists agreed that there was a residuum of science, which I called intrinsic basic research, that was pursued for no reason except the advancement of science itself. We all recognize this to be an essential part of science, but we conceded its support did not easily fit into the responsibilities of the nonscience agencies.

At that time, since it appeared that the budgets of strong science supporting agencies like the Atomic Energy Commission and the Defense Department and NASA indeed were diminishing, we argued that NSF should pick up the support of this residuum insofar as possible and in fact in this book was coined the idea of NSF being a balance wheel to move in when the mission-oriented agencies couldn't pick up their share of this basic research.

How much of what we discussed and argued 15 and 20 years ago is relevant today? Three new elements have been injected into the debate:

First, a reduction in the real budget of most nondefense-related agencies; second, a growing belief, expressed most succinctly by the economist Simon Rottenberg in his article "The Economy of Science: The Proper Role of Government in the Growth of Science," as he will explain, that the private sector ought to support most of the science necessary to achieve nonscientific objectives. Rottenberg argues that in most cases Government agencies spend money poorly when they support science that is not unequivocally connected to achievement of their missions, and third, that the United States is now engaged in rearmament.

From the perspective of 15 years ago, our response to the reduction in real budget of most agencies except Defense would have been, raise NSF's budget to serve as a balance wheel. But if I understand the current situation, NSF is not in a position to serve as a substantial balance wheel. Judging from the testimony given before this committee last year, one gets the impression that though the very best scientists in the very best centers will continue to receive support, scientists in less advantaged parts of the country will suffer.

Coming from Tennessee, that 40 years ago was a scientific desert but which now boasts lively, high-technology industrial spinoff, I cannot concede that geographic distribution of what remains of the science budget is less important than support of the very best, but this of course is an arguable point.

Nor can I agree that the mission agencies should stop supporting relevant science because private industry, being disciplined by the market, will apply stricter and more sensible criteria for support than can Government.

Professor Rottenberg concedes that where the aim to be achieved is not mediated by the marketplace, as in defense or even environmental protection, the Government has a role to play in supporting the underlying science. But where the market operates, the less government involvement—even in the underlying science, and this is the argument that I think is presented in his article—the better.

Unfortunately, the nonmarket externalities are never all that clear. For example, to take an issue very much in Congress mind, the breeder, not an uncontroversial matter, can be viewed simply as an alternative way of producing electricity and ought to be developed, if at all, by the utilities, perhaps through their joint research organization, the Electric Power Research Institute.

I would argue that one of the ultimate arguments for the Government being interested in the breeder is the question of oil imports and the question of carbon dioxide. These are examples of externalities that are not captured by the marketplace.

I will skip my discussion about the overall health of the economic system being in a sense an externality that is not perceived by the individual company, but simply point out to you that we are in an economic competition with Japan where the Government and the industry through MITI are very much in cahoots with each other.

Let me touch on defense. Regardless of one's attitude toward the events in Lebanon, the overwhelming technical superiority of the largely American-equipped Israeli air force over the Soviet-equipped Syrian air force is a stunning reality. This vast technical advantage is a distillation of the scientific and technological power of the United States and of Israel, and illustrates the inseparability of strong defense and strong science and technology.

If we are to move away eventually from dependence on tactical nuclear weapons for deterrence in Western Europe, we shall have to depend on more sophisticated conventional defense systems. These will surely be based on better, more intelligent technology. In short, the Israeli shut-out against the Syrian air force demonstrates again how heavily our defense posture is dependent on science and technology. The moral must be clear: This sophistication does not spring up in isolation. It is imbedded in a flourishing, active scientific enterprise at every level.

I therefore echo the view expressed that the Department of Defense, if it has serious interest in maintaining this overall capability in science and technology, perhaps might serve as a sort of balance wheel for much research that we in 1965 had envisaged to be the province of the National Science Foundation.

Before NSF was established, there was the Office of Naval Research which served as one of the major governmental patrons of science and in this period when support for science is diminishing, the possibility of DOD, or at least in part, taking on this historical function of being a sort of a balance wheel I think deserves serious attention by Congress and the executive branch.

Thank you very much, Mr. Chairman.

[The prepared statement of Dr. Weinberg follows:]

SETTING PRIORITIES ON SCIENCE*

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I am pleased, and flattered, to have this opportunity to give my views on priorities in science. I should make clear that, though I participated in the debate on priorities during the 1960s, I have been mostly an interested spectator in recent years. What I can do is give some historical perspective; and then present a few personal impressions as to where the debate stands today.

The Historical Context

Bruce Smith's paper, "The Concept of Scientific Choice: A Brief Review of the Literature," which has been distributed to the two Subcommittees, remains an excellent survey of where the debate on how to allocate resources for science stood in the mid-1960s. In a way, I have a feeling of déjà vu: the issues that trouble science policymakers today--how much should be allocated to science in comparison to other activities of government; and how should resources allocated to science be divided among different scientific fields--remain much the same as those debated 20 years ago. This should give us a sense of humility: We cannot expect to get revealed wisdom, true and correct for all time, on the issue of priorities in science. Each generation of policymakers will address essentially the same questions, but always with a concern for practical exigencies of the time. Today those exigencies are the strong reduction in non-defense elements of the Federal budget, and the even stronger increase in expenditures for defense.

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*Presented before the Subcommittee on Investigations and Oversight, and the Subcommittee on Science, Research and Technology, U.S. House of Representatives Committee on Science and Technology, Washington, D.C. September 30, 1982.

I became involved in the debate on priorities when I was a member of the President's Science Advisory Committee from 1959 to 1962. At the time, the central question in science policy was whether or not the United States should go to the moon. President Eisenhower had decided against the manned moon mission, a decision reversed by President Kennedy. This decision brought into strong focus the whole conceptual underpinning for making choices in science. At the same time, Professor Edward Shils, the distinguished sociologist of the University of Chicago, had founded the journal Minerva: one of its purposes was to provide a forum for the scholarly and philosophic debate over how to set priorities within science. Indeed, the very first article in Minerva, issued in Autumn 1962, was entitled "The Republic of Science: Its Political and Economic Theory," by the late distinguished physical chemist, Michael Polanyi. In this very influential article, Polanyi likened the collective actions of all scientists to an interacting economic and political "republic" which is governed, in analogy with ordinary economic activity, by what Harvey Brooks calls the "intellectual marketplace." In Polanyi's view, because of the intrinsic impossibility of foreseeing where science is going, the invisible hand, operating by countless competing scientists and scientific doctrines, provides the best way of allocating resources. Good science and good scientists inevitably pushes — ? out bad science and bad scientists. In short, the free market works far better for allocations in science than does any form of socialism: Polanyi's position may almost be described as a reduction of science policy to a branch of economics, with the intellectual marketplace doing the allocating.

My first article, "Criteria for Scientific Choice," conceded that Polanyi's Republic of Science worked tolerably well for little science--i.e., science done in more or less a traditional way by individuals or small groups whose demands on the budget, taken individually, were not very large. But for the really large decisions--whether to build a 1000 GeV accelerator or a space telescope--I suggested that criteria internal to science and derived from the intrinsic logic of the science were insufficient. For such decisions, criteria external to science, or to a given field of science, were necessary. I described these as technological merit, social merit, and scientific merit: in each instance, the value of the proposed science is measured by its effectiveness in achieving an end that arose from outside the science: say, a technological aim, like fusion; or a social aim, like military strength; or, in the case of scientific merit, progress in neighboring fields of science.

My criteria of choice amounted to setting up a system of scientific value. Philosophers of science have always been concerned with scientific truth: they wrestle with the question of how to test the truth of a scientific finding. I was asking a different question: of two scientific findings, both of which are equally true, how does one decide on which is the more valuable? I called such inquiry the "axiology" (as opposed to the "epistemology") of science. I argued that since the basic problem of scientific administration was to allocate resources between competing scientific claimants, that an axiology of science ought to be useful to the scientific administrator. Thus I considered the axiology of science to be a sort of philosophic underpinning for scientific administration in the same way as more traditional epistemology serves as an underpinning for science itself.

In the intervening years, several attempts have been made to apply these scholarly speculations to actual allocations in science. Perhaps most notable were a series of reports put out by the National Academy of Sciences in which broad fields of science were assessed. The most ambitious report was the one on physics carried out under the chairmanship of Professor Allen Bromley of Yale University in 1972. The Bromley Committee used explicit criteria, largely based on those I had delineated, to rank the various sub-fields of physics. Their top three sub-fields and institutions of physics--lasers and masers, quantum optics, and elementary particles--today continue to receive very strong support. On the other hand, their next highest ranking field, nuclear physics, no longer seems to receive as much support as the Bromley Committee recommended.

Basic Research and National Goals

The debate on allocations for science probably reached its zenith in 1965 with the report, "Basic Research and National Goals." This study, chaired by George Kistiakowsky, a former science adviser to the President, consisted of 15 separate essays by members of the panel. The entire panel criticized each essay, though each essay was the responsibility of its author, and each author was allowed to accept or reject the criticism. The essays were introduced by a longish summary that I wrote with the help of Professor Kistiakowsky.

As one would expect, "Basic Research and National Goals" covered the whole range of opinions as to how much science ought to be supported (from a high of 10-15 percent growth per year, to economist Harry Johnson's view that "the argument that individuals with a talent for such research should be supported by society...differs little from arguments formerly advanced in support of the owners of landed property to a leisured existence...insistence on the obligation of society to support the pursuit of scientific knowledge for its own sake differs little from the historically earlier insistence on the obligation of society to support the pursuit of religious truth, an obligation recompensed by a similarly unspecified and problematical payoff in the distant future.").

My own views began with a rejection of science as a whole as a valid category. Instead, science that was clearly relevant to achievement of an applied aim ought to be viewed as an overhead, assessed not against some fictitious scientific budget, but rather against the budget for achievement of that end; for example, research on lasers relevant to design of anti-tank weapons ought to be charged to anti-tank defense, not to science. Thus every agency of Government ought to allocate some of its budget to the use of science in the achievement of its aim--just as every corporation allocates some of its budget to scientific research because it believes that research will pay off in better products or services. What that fraction allocated to research is, cannot be judged a priori--and, to be sure, it will fluctuate as the priority for the underlying non-scientific aim fluctuates.

In addition to the science more or less directly tied to the achievement of non-scientific aims, and therefore properly supported by Government agencies such as Agriculture or Interior or Defense (that regard science simply as a means to an end, not an end in itself), most of our panelists agreed that there was a residuum of science (which I called Intrinsic Basic Research) that was pursued for no reason except the advancement of science itself. We all recognized this to be an essential part of science--but we conceded its support did not easily fit into the responsibilities of the non-science agencies. I concluded at the time (1964) that support of such science was almost the central question--and that it was connected strongly with the role and strength of the National Science Foundation (NSF). In view of what seemed to be a retrenchment of budget in Defense, the Atomic Energy Commission and the National Aeronautics and Space Administration, I, along with many other panelists, supported the notion of the NSF as balance wheel--to take up the slack when fluctuations in support by the scientific arms of the mission-oriented agencies fell in response to the change in national priorities.

The Present Scene

How much of what we discussed and argued 15 and 20 years ago is relevant today? Three new elements have been injected into the debate: First, a reduction in the real budget of most non-defense related agencies; second, a growing belief, expressed most recently by the economist Simon Rottenberg in his article, "The Economy of Science: The Proper Role of Government in the Growth of Science," (Minerva, Autumn 1982) that the private sector, guided in its allocations by the market, ought to support most of the science necessary to achieve non-scientific objectives. (Rottenberg argues that in most cases, Government agencies spend money poorly when they support science that is not unequivocally connected to achievement of their missions.) And third, that the United States ought to re-arm.

From the perspective of 15 years ago, our response to the reduction in real budget of most agencies except Defense would have been, raise NSF's budget to serve as a balance wheel. But if I understand the current situation, NSF is not in a position to serve as a substantial balance wheel. Judging from the testimony given before this Committee last year, one gets the impression that though the very best scientists in the very best centers will continue to receive support, scientists in less advantaged parts of the country will suffer. Coming from a part of the country that, 40 years ago was a scientific desert, but which now boasts lively, high-technology industrial spin-off, I cannot concede that geographic distribution of what remains of the science budget is less important than support of the very best.

Nor can I agree that the mission agencies should stop supporting relevant science because private industry, being disciplined by the market, will apply stricter and more sensible criteria for support than can Government. Professor Rottenberg concedes that where the aim to be achieved is not mediated by the marketplace--as in defense or even environmental protection--the Government has a role to play in supporting the underlying science. But where the market operates, the less Government involvement, even in the underlying science, the better. Unfortunately, the non-market externalities are never all that clear. For example, many would argue that the breeder, being simply an alternative way of producing electricity, ought to be developed, if at all, by the utilities, perhaps through their joint research organization, the Electric Power Research Institute. Let the manufacturers and sellers of electricity decide on the appropriate discount rate to be used in planning and developing a totally new source of electricity. But the utilities are not concerned, in any direct way, either with the problem of oil imports, or the possibility of a carbon dioxide-induced change in climate. Both are externalities that Government must be sensitive to; both considerations point to continuing development of the breeder by the Government, as a means both of reducing oil imports and of hedging against CO_2 build-up--even though strict market economics would militate against the development.

I realize that the breeder is an extreme case--where the externalities are hardly a matter of dispute, and are sufficiently powerful to justify strong Government intervention. In other cases, the externalities may appear to be more contrived--as, for example, in research aimed at producing a safer automobile, or even a leak-proof tanker. Obviously the cases are almost never clear-cut. But I would remark that even in the commercial sphere, the United States suffers from foreign competition, particularly Japanese; and much of Japan's advantage flows from the intimate government-industry involvement in research carried out through MITI. Though the health of a specific company is not generally a government concern here, the health of our entire industry in a fiercely competitive world is a governmental responsibility. Thus there are economic externalities--health of the entire economy--not easily captured by individual industries themselves, and this argues for a liberal, rather than restrictive interpretation of the extent to which externalities justify scientific intervention by government.

Finally, I will touch upon Defense. Regardless of one's attitude toward the events in Lebanon, the overwhelming technical superiority of the largely American-equipped Israeli air force over the Soviet-equipped Syrian air force is a stunning reality. This vast technical advantage is a distillation of the scientific and technological power of the United States and of Israel, and illustrates the inseparability of strong defense and strong science and technology. If we are to move away eventually from dependence on tactical nuclear weapons for deterrence in Western Europe, we shall have to depend on more sophisticated conventional defense systems. These will surely be based on better, more intelligent technology--better computers, lasers, communication systems, to mention a few. In short, the Israeli shut-out against the Syrian air force (86-0) demonstrates again how heavily our defense posture is dependent on science and technology. The moral must be clear: this sophistication does not spring up in isolation. It is imbedded in a flourishing, active scientific enterprise at every level--the very best, the competent, the learners, the young. Thus, just as the viability of a steel industry and an auto industry and an airplane industry is of central concern to the defense of our country, so I would argue is the continuing strength of our scientific enterprise--in every dimension, not merely in the aspects that are clearly and immediately relevant to defense. I would therefore echo the view expressed at last year's meeting that the Department of Defense, with its serious interest in maintaining this capability, might serve as a sort of balance wheel for much research that we, in 1965, had envisaged should be the province of the National Science Foundation. One must remember that before NSF was established, immediately after the war, the Office of Naval Research served as one of the major governmental patrons of science. In this period when support for science from many of the non-defense agencies is diminishing or remaining static, the possibility of the Department of Defense renewing, at least in part, this historical function deserves serious attention by Congress and the Executive Branch.

Mr. **Brown**. Thank you for that very stimulating review, Dr. Weinberg.

Dr. Rottenberg?

STATEMENT OF DR. SIMON ROTTENBERG, PROFESSOR OF ECONOMICS, UNIVERSITY OF MASSACHUSETTS, AMHERST, MASS.

Dr. **Rottenberg**. Setting priorities for Government support for scientific research involves finding responses to a number of questions. What kind of scientific research should receive Government support and what kind should not?

For the research that is Government supported or is appropriately Government supported, what is the appropriate scale of that support? How much should be given to scientific research as opposed to other objectives of Government expenditure?

Within the rubric of Government-supported scientific research, how should support be allocated among fields, projects, research institutions, and researchers? The answers can be sought by perceiving scientific research as an industry or economic activity.

Like other industries, the activity that we call scientific research, employs resources having alternative valuable uses to society; it is an investment activity in the sense that the costs of the activity are incurred in the present but the payoffs come in the future. It is a risky activity in the sense that the research may fail to discover or the discovery may become quickly obsolescent.

For most industries there is no subsidy and there is no central direction with respect to scale and methods. Whether a commodity is produced or not, how much is produced, and the methods of its production are determined by the application of a market test. An activity is engaged in if it will yield a normal return on investment and the methods chosen are the lowest cost methods.

Market tests are the appropriate method for responding to the priority questions, if the market is competitive, that is to say, if there is freedom to enter and exit, and if an investor in an activity appropriates or captures all of the gains of the activity. This is so because markets are efficient in the aggregation of information about social preferences and social costs.

The market is the appropriate instrument for determining whether applied research should be done, how much applied research should be done, in which fields applied research should be done, and how applied research should be carried out.

Applied research is that which seeks to discover the application of knowledge to some practical use. Explicit estimation of the magnitudes of the variables affecting whether applied research of a particular kind should be done is enormously difficult. They include estimates of the probability that research will be successful, the values of discoveries, and the length of the period before discovery becomes obsolete.

Explicit estimation by Government officeholders should be avoided, if the market can serve as an alternative instrument for making applied research decisions.

If Government support is given for applied research, there will be an excess of resources devoted to the activity.

The rate of return on the activity will be below the normal rate of return; some resources used in applied research will have better uses for society in other activities. There is one exception to the market test rule for applied research: Where the buyers of applied research, as in the case, for example, of applied research for defense, is a single agency, or very few agencies, Government support is indicated. In this case, the market will fail.

If there is only one purchaser of submarines, they will not be built, except under explicit contract. If there is only one purchaser of research intended to make submarines more efficient, it will not be done, except under explicit contract.

The risk of estimating defense procurement policy is too large for commercial firms to undertake those activities, unless they are reimbursed by contract arrangements for doing so. Actually, Government support for applied scientific research extends far beyond those limits.

The 5-year outlook, which defines plans or aspirations for Government support for research, includes a large number that fit an applied research set.

Basic research, which produces increments of pure knowledge about nature, man, and society, with no known or intended practical use should, however, be Governmentally subsidized.

This is because basic research produces what economists call a public good. None can be excluded from the consumption of increments of pure knowledge, and the consumption of a unit of that knowledge by some leaves undiminished the quantity of it available to others.

In the case of public goods, the market fails. If market processes were to determine the scale of output of increments to the stock of pure knowledge, there would be underinvestment in the activity and the rate of growth of the stock would be lower than is socially appropriate. Government should, therefore, support basic research.

Since it does not seem possible to measure the value of pure knowledge of nature that has no intended applied use, economic principles of organization may offer very little practical guidance in determining the quantity of society's resources that should be devoted to basic research.

Not only does it not seem possible to value the output of pure knowledge, but it seems also not possible to know what will be discovered by those engaged in basic research, how long it will be before research discovery occurs, how long the discovered knowledge will survive before it becomes intellectually obsolescent, and the rate at which it is appropriate to discount the services that the discovery performs in giving society deeper understanding of aspects of nature.

Although a governmental program of support is indicated for basic research that will move some resources into basic research from other activities in which they would have been employed, care should be shown that such a program is not too large.

Too much should not be made of the defense for an active and extensive public science policy in terms of service as a public good. Exclusion is possible for patentable discoveries and, for centuries, persons engaged in scientific research have made unpatentable or unpatented discoveries and released them to the general community. Such people are rewarded in the currency of professional repute and the respect of their peers. Indeed, rewards of that sort cannot be procured unless discoveries are revealed.

Some measure of basic research goes on in institutions of higher learning without public subvention. Their faculties are expected to be engaged in joint ventures in which they both communicate already discovered knowledge to their students and also seek to add increments to the stock of knowledge. They are paid, if they are successful, in higher lifetime income streams and by invitations to join institutions that are characterized by intellectually more interesting dialogue.

Assuming that these incentives are insufficient to secure a socially optimal scale of basic research investment, it is the proper specialized province of Government to invest in such research.

Just as Government lacks the capacity to measure the value of increments of pure knowledge in the aggregate, it also lacks the capacity to measure the relative values of different increments of pure knowledge that are produced by basic research work in different fields. It cannot explicitly rank-order the increments of knowledge of the constituent components of basic research, nor can it assign them weights that indicate their comparative values to society.

Government should avoid, if it can, measurement tasks that it cannot execute well.

There is a possible method that might solve the problem of allocating governmental funds for support of basic research while avoiding explicit estimation of the values of the output of research activities. That method implies choosing among scientists rather than choosing among scientific fields and projects.

It is a method that can be derived implicitly from the late Prof. Michael Polanyi's discussion of the intellectual marketplace. In that marketplace, the scientific community contrives professional standards for determining scientific merit. The authority of scientific opinion enforces professional standards.

Polanyi's perception offers a method for the allocation of governmental resources for basic scientific research that would permit governmental officeholders to avoid the explicit estimation of cost and gains. They would allocate not among fields but, rather, among scientists. They would inquire about the qualities of intelligence and of the professional achievement or promise of scientists in the queue, but would not ask about the relative values of the various and different discoveries of pure knowledge they seek to achieve. They would not seek to refashion the structure of pure scientific work but would permit its structure to emerge spontaneously from the unconstrained choice of individuals among scientific careers and scientific projects.

By applying the method of searching out scientists who have merit, as measured by the consensual judgment of the scientific community, and by permitting them to work on problems that rouse their curiosity and are responsive to their sense of what the scientific community considers to be important, the Government might make a stronger contribution to the advancement of science and the progress of knowledge than it would if it attempts the explicit estimation of the relative values of scientific discoveries.

Mr. **Brown**. Thank you very much, Dr. Rottenberg.

[The prepared statement of Dr. Rottenberg follows:]

Statement of Simon Rottenberg, Professor of Economics, University of
Massachusetts, Amherst

Committee on Science and Technology, U. S. House of Representatives,
September 30, 1982

A search for the principles that define the proper role of government in the support and promotion of science and for the practices that will cause those principles to be applied is similar to the exercise in which economists engage when they attempt to solve the problem of allocating resources among alternative uses so that social welfare will be maximized.

Scientific research consumes resources. Those resources have alternative uses which have value for society. Resources are scarce add their use in research imposes an opportunity cost upon society by forestalling their use for other purposes. Resources that are put to any particular use in scientific research also impose opportunity costs because they are then not available for other scientific investigations.

Research is an investment. If it is successful, there is a lag between the time when the research is begun and carried on and the time when successful discovery occurs.

Research is a risk; some are successful and some are failures; there are different degrees of success and of failure.

As in any other venture that seeks to produce something, there are, in research, alternative processes and methods of production that can be employed.

The "economic problems" that every society somehow solves - sometimes well and sometimes badly - are: What (and how much) to produce?, and How to produce?

Economics has developed a set of "maximizing conditions" that assure, if they are fulfilled, that those problems will be solved in ways that optimize for society.

They are, however, conditions that are extraordinarily difficult to fulfill explicitly, in the case of scientific research activity that warrants governmental support, because the product of that activity is very difficult - and possibly impossible - to measure.

Three fundamental questions confront the design of public science policy. They are:

Under what circumstances is it appropriate for government to finance scientific research and when, alternatively, should financing come from other sources?

What is the appropriate scale or size of government support for the scientific research that it should support?

How should government support for scientific research be allocated among fields, projects, and researchers?

Unless there is strong evidence of market failure, competitive, commercial markets should be relied upon to determine the scale of scientific research, its distribution among fields and projects, and the methods by which production of research outcomes occur.

Although the explicit calculation of the values of the variables that are relevant to the fulfillment of social optimality conditions is difficult or impossible, the market will tend to cause them to be fulfilled and socially efficient outcomes to occur. The market is an efficient instrument for deciding the uses to which the resources of society should be put. This is because the market is efficient in the aggregation of information and in the construction of social evaluations from the differences among individuals who compose society in the values they put upon commodities, services, risk, uncertainty and postponement and in the estimation of the future.

Generally, markets will not fail in deciding how much applied research should be done, which applied research should be done (and which not), and who shall do it and where it should be done.

Markets will make those decisions correctly and efficiently, if those who make applied research investments can appropriate or capture all the gains generated by successful research investments. If they are successful, they will recover their investment costs and a"normal" return on their investment, adjusted for the risk of failure.

"Applied research" is directed towards making usable discoveries toward the practical application of already acquired knowledge.

Government should not, in most cases, finance applied scientific research. If it does, there will be an excess of investment in that activity and some social resources will have been put to less than their most highly valued uses. Not only will the scale be wrong, but, because it is not possible to assimilate and manipulate the vast and diverse quantity of information that will permit the estimation of the aggregated preferences of all individuals, households, and firms in the society, research resources will be inefficiently allocated among fields,projects, and researchers.

The government has supported and does support applied scientific research heavily, and a reading of the volumes of The Five Year Outlook, published by the National Science Foundation, reveals numerous aspirations for research support of the executive agencies that have applied research content.

There are three cases in which markets will fail and, in the absence of government subsidies, there will be underinvestment in activities.

They are the cases of activities that produce public goods, those that produce external benefits, and those which produce a commodity whose sole purchaser is a (or a very few) government agencies.

A "public good" has the properties that none can be excluded from its consumption and that the consumption of a unit of it does not diminish the quanity of it that may be consumed by others.

(A lighthouse is the classic example. No passing ship can be excluded from acquiring the information that the lighthouse marks dangerous shoals; any ship acquiring this information leaves undiminished the quantity of information that is available to other passing ships.)

An "external benefit" is a gain appropriated by others than those who engage in an activity or a transaction.

Since investors who bear the whole cost of an investment will, when deciding whether to engage in an activity and deciding upon the magnitude of ~~the~~ scale of the activity, take account of all the costs they bear and the benefits they, themselves, will reap but will take no account of the gains of others, the market will generate underinvestment in activities that produce public goods or external benefits.

To secure a socially optimal scale of such activities requires that they activity be subsidized.

Basic scientific research is such an activity. "Basic scientific research" seeks more fundamental knowledge of a phenemenon, but not its practical application.

Basic research clearly produces a public good. None can be excluded from the consumption of the incremental knowledge it produces and the acquisition of that knowledge by one does not diminish the quantity of it that is available to others.

If the production of basic scientific research were a private market activity, too little of it would be done. Those who bore its cost would not be able to appropriate the gains of their discoveries, when they were successful. Government should, therefore, finance basic research.

The third case in which governmental financing of research is indicated, because of market failure, is that in which the discoveries produced by the research have value for a single, or a few, governmental agencies. Although private firms will produce pencils and shoes and carry inventories of them, waiting for buyers to come, none will produce submarines, except on explicit contract. The two cases are differentiated by the number of buyers - a multiplicity of them in the case of pencils and a single public sector agency in the case of submarines. Similarly, none will invest in research, and even applied research, into methods for making submarines more efficient except upon explicit contract.

In considering governmental support for basic research, it is important to understand that scientific knowledge is, in itself, a useful product. It is a public consumption good. It is appropriate to use some of the community's resources to enlarge the stock of knowledge for its own sake. Basic research that yields pure knowledge is valuable for society.

Too much should not be made of the defense for an active and extensive public science policy in terms of service as a public good. Exclusion is possible for patentable discoveries and, for centuries, persons engaged in scientific research have made unpatentable or unpatented discoveries and released them to the general community. Such people are rewarded in the currency of professional repute and the respect of their peers. Indeed, rewards of that sort cannot be procured unless discoveries are revealed.

Some measure of basic research goes on in institutions of higher learning without public subvention. Their faculties are expected to be engaged in joint ventures in which they both communicate already-discovered knowledge to their students and also seek to add increments to the stock of knowledge. They are paid, if they are successful, in higher lifetime income streams and by invitations to join institutions that are characterized by intellectually more interesting dialogue.

Assuming that these incentives are insufficient to secure a socially-optimal scale of basic research investment, it is the proper specialized province of government to invest in such research.

Public policy then confronts the problem of defining the scale of governmental investment in the activity. The gains are real, but it is difficult to measure their value.

How is one to measure the values to society of somewhat deeper understanding, for their own sakes, of the emission of energy by quasars, immune processes at cellular and molecular levels, the synthesis of polymers from nonpetroleum resources, the electronic structure of an ordered solid, and the processes of chemical migration in the earth's crust in comparison with the values of known commodities and services to the production of which resources employed in basic research might have been put?

In principle, the appropriate quantity of basic research is that which will cause the rate of return on investment in that activity to be the same as the rate of return on investment in other lines, adjusted for risk.

The rate of return on investment in private market economic activities is a rough approximation of the rate of return to be sought from public investment in basic research. The application of the rule requires the estimation of the cost of basic research, of the gains from the increments of knowledge it produces, of risk, and of the discount rate. It is a difficult rule to apply explicitly and the task of applying it should be sedulously avoided by governmental officeholders by permitting the market to allocate resources to research and among research fields, except where a strong case can be made that market failure will occur.

Since it does not seem possible to measure the value of pure knowledge of nature that has no intended applied use, economic principles of organization may offer very little practical guidance in determining the quantity of [illegible] that should be devoted to basic research.

Not only does it seem not possible to value the output of pure knowledge, but it [illegible] possible to know what will be discovered by [illegible] research, how long it will be before research discovery occurs, how long the discovered knowledge will survive before it becomes intellectually obsolescent, and the rate at which it is appropriate to discount the services that the discovdry performs in giving society deeper understanding of aspects of nature.

Although a governmental program of support for basic research that will move some resources into basic research from other activities in which they would have been employed is indicated, care should be shown that such a program is not too large.

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Just as government lacks the capacity to measure the value of increments of pure knowledge in the aggregate, it also lacks the capacity to measure the relative values of different increments of pure knowledge that are produced by basic research work in different fields. It cannot explicitly rank-order the increments of knowledge of the constituent components of basic research nor can it assign them weights that incidate their comparative values to society.

Government should avoid, if it can, measurement tasks that it cannot execute well.

There is a possible method that might solve the problem of allocating governmental funds for support of basic research while avoiding explicit estimation of the values of the output of research activities.

That method implies choosing among scientists rather than choosing among scientific fields and projects.

It is a method that can be derived implicitly from the late Professor Michael Polanyi's discussion of the intellectual marketplace. In that marketplace, the scientific community contrives professional standards for determining scientific merit. The authority of scientific opinion enforces professional standards.

Scientific standards are, Polanyi says, uniform throughout science. This "makes possible the comparison between the value of discoveries in fields as different as astronomy and medicine. . . . (It is) the principle which underlies the rational distribution of grants for pursuit of research. . . So long as each allocation follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole." (Michael Polanyi, "The Republic of Science", in Minerva, I, 1 (Autumn, 1962, pp. 54-73).

Polanyi's perception offers a method for the allocation of governmental resources for basic scientific research that would permit governmental officeholders to avoid the explicit estimation of costs and gains. They would allocate not among fields but, rather, among scientists. They would enquire about the qualities of intelligence and of the professional achievement or promise of scientists in the queue, but would not ask about the relative values of the various and different discoveries of pure knowledge they seek to achieve. They would not seek to re-fashion the structure of pure scientific work but would permit its structure to emerge spontaneously from the unconstrained choice of individuals among scientific careers and scientific projects.

By applying the method of searching out scientists who have merit, as assessed by the consensual judgment of the scientific community, and by permitting them to work on problems that arouse their curiosity and are responsive to their sense of what the scientific community considers to be important, the government might make a stronger contribution to the advancement of science and the growth of knowledge than it would, if it attempts the explicit estimation of the relative values of scientific discoveries.

Mr. BROWN. Let's have Dr. Hill now present his statement.
[The biographical sketch of Dr. Hill follows:]

DR. RICHARD J. HILL

Dr. Richard J. Hill is Provost and Vice President for Academic Affairs at the University of Oregon in Eugene, Oregon, where he was formerly Head of the Department of Sociology, Director of the Institute for Social Research, and Dean of the Wallace School of Community Service and Public Affairs. Hill receive his B.A. (1950) and his M.A. (1951) from Stanford University, and the Ph.D. from the University of Washington in Seattle (1955) where he was also a staff member of the Public Opinion Laboratory.

Subsequently he was a member of the staff of Bell Telephone Laboratories In New York City and on the faculties of the University of California at Los Angeles, the University of Texas at Austin, and Purdue University in West Lafayette, Indiana. He has been a consultant for the National Science Foundation and on the advisory board of its Science for Citizen's Program. He also served several terms on review panels for the National Institute of Mental Health.

Hill has been the editor of Sociometry and has served on the editorial boards of several other professional journals. His published work is mainly concerned with research methods, the attitude-behavior relationship, the effects of mass communication, and professional socialization. Hill has been elected to a number of professional offices including the presidency of the Pacific Sociological Association and the Council of the American Sociological Association.

STATEMENT OF DR. RICHARD J. HILL, PROVOST AND VICE PRESIDENT FOR ACADEMIC AFFAIRS, UNIVERSITY OF OREGON, EUGENE, OREG.

Mr. Hill. Thank you, Mr. Chairman.

Being sixth on a list of experts makes much of what I have to say redundant, I am afraid.

Mr. Brown. Read fast in that section.

Dr. Hill. Thank you, sir.

The process of resource allocation in an open, pluralistic society becomes increasingly critical when available resources diminish. What seems to be most vexing to many of my scientific colleagues is the apparent irrationality of the allocation process. The process is not well understood. It is not even well described.

Clearly, in the public sector, such decisions are political and are derived from social and economic assumptions that change periodically and are never universally acceptable. Thus, we find ourselves in the totally predictable situation where the process becomes an issue and where those involved in setting priorities become suspect.

It is clearly in my self-interest to urge the Congress to increase support for scientific research and development. The multitude of problems within the scientific enterprise have been well described this morning.

Under existing political and economic conditions, the arguments advanced by the scientific communiuty for increased general levels of Federal support probably will not carry the day. Thus, at least in the immediate future, the allocation of existing resources becomes increasingly critical.

If I am correct, the process of allocating public resources is essentially political, and we must expect the assessment of probable social and economic payoff to be an important consideration. That payoff is manifested in the applications of science to the solutions of problems and in the development of new technologies.

I believe that at any moment in the total development of science, we can make very good guesses about those areas of activity likely to have the most immediate payoffs. Judgments of this sort have to do with what has been referred to as the ripeness of science. One of the experts on the ripeness of science is sitting to my right.

An area of science reaches this stage only when a great deal of work has been accomplished and, perhaps more critically, when the importance of what is not known is recognized.

It seems clear, for example, that molecular and cellular biology have reached the ripe stage. It also is relatively easy to predict that one of the next stages of the high-technology revolution will be in the area if bioengineering. Further, the social and economic consequences of these developments are likely to be of major significance in areas as diverse as medicine and forestry. To the extent that payoff is a consideration, the allocation of resources to these areas is increasingly a low-risk venture.

It is important to recognize that when an area of science reaches this stage of maturity the resource base is likely to change, with the private sector becoming an increasingly important source of support. The developments in microelectronics clearly illustrate the principle. As such developments increase, the availability of public funds becomes less crucial for the continued progress of developmental activity. Some public resources will still be required for basic research, but the funding of applied research and development can be assumed by the private sector.

To the degree that relatively immediate social and economic consequences are a concern, judgments about the maturity of an area of scientific inquiry become important. Here I believe that the scientific community is the only segment of our society qualified to perform the task. Scientists, at least some of them, do think and talk about the maturity of their fields.

For example, in recent conversation with some of my colleagues, the judgment was made that both the neurosciences and the psychobiology of cognition were nearing that stage of maturity where important new applications could be expected. If that judgment can be validated, then the investment of public funds to bring these areas of inquiry through that next stage of development is clearly a reasonable decision.

I believe that it is possible to establish a mechanism to provide such assessments of the various areas of scientific inquiry on a continuing basis. Scientists will disagree, for they, too, suffer from the biases introduced by self-interest. Nevertheless, a body like the National Academy of Sciences could assume the function of providing Congress with an ongoing assessment of the ripening process.

Assigning high priority to areas of science nearing maturity will have costs to other areas of scientific activity if the total resources available are not increased. Here we directly confront some very hard decisions.

The Federal Government must continue to be the major source of support for basic research in disciples that have not reached the stage of maturity of, for example, the physical chemistry of silicons, short-term payoffs are not apparent and significant support from the private sector should not be expected. The long, tedious, and costly process by which a discipline reaches maturity must not be abandoned.

We need to establish a balanced and stable degree of support for basic research across the range of scientific disciplines. We should recognize that until conceptual, factual and technical maturity are achieved in a field, applications are likely to have limited consequences, and such work should occupy a position that is lower on our list of priorities. Support of basic research should be seen as a long-term public investment, an investment that has paid excellent dividends in one area after another as these areas have matured.

The central theme in the above is that in the areas of basic research scientists themselves should be relied upon to set priorities both within and between areas of inquiry.

As we move toward application and technology, the situation changes. It is in its application that the Jekyll and Hyde character of science becomes apparent. A drug that provides tranquility also results in malformed infants. We are having other troubles, troubles that this subcommittee is well aware of. The public is concerned about these issues, and I have been asked to comment specifically on public involvement in the process of resolution.

The public demand for protection against the Love Canals and the Three-Mile Islands will not rapidly dissipate, nor should it.

This demand requires that we give high priority to increasing our resources for the general assessment of applied science and technology. We must develop further our ability to monitor, to collect adequate longitudinal data, and to test systematically the byproducts of applied science. The kind of scientific research required is lowgrade, often routine, and scientifically not very challenging, but it is essential.

It would be difficult to establish such a monitoring system within the current structure of university research. We should not assign the task to the private sector for obvious reasons. The problems involved are not strictly State or local in nature. For example, toxicity and its producers migrate. Thus, monitoring functions, I believe, must be performed within and supported by the Federal Government. Clearly, some such activity now is under way, and I do not intend to minimize the significance of such agencies as the Office of Technology Assessment.

I do believe that the resources for scientific monitoring need to be increased and better coordinated. Certainly public trust in such activity needs to be improved far beyond current levels.

The question of whether or not the public should be involved in influencing research and development priorities is not really relevant. Important segments of the public are now involved, and in future discussion of the matter, this should be taken as a given. Kenneth Prewitt's recent discussion entitled "The Public and Science Policy" is suggestive. Prewitt treats the notion of specialized and attentive publics. These publics are self-selective, well-informed and organized. Some of these public emerge as single-issue forces that agitate against various science and technology developments but there is also evidence of an attentive science public that is generally sympathetic toward science and vocal in its support.

Much of the public's involvement to date has had the character of protest, frequently leading to public hearings and, at least occasionally, to litigation. If we live in a period of technological revolution, we also live in a litigious age, and if our pluralistic society is to be governed by law, we should expect this litigious age to be a long one.

The costs involved in existing advocacy procedures bring me back to the monitoring process discussed above. Effective scientific monitoring could expedite review and facilitate decisionmaking, if the monitoring process gained public confidence. Thus, those who would argue that such increased monitoring would constitute costly overregulation should compare the potential of such a system against the costs of current regulatory policies.

I have argued that if such monitoring systems are to be effective, they must gain the confidence of the public. Brooks' discussion of such systems includes an important suggestion:

> Any monitoring or data collection system should be subject to a continuing process of review, assessment, and possible revision by outside advisory groups, including scientists, laymen, and policy-makers. The review should be much broader in its disciplinary representation than the customary peer review panel or research advisory committee to an agency.

That is Mr. Brooks' statement.

I agree. We need to monitor the monitoring systems and in this task I am convinced that members of the attentive public can participate in a meaningful and constructive fashion. Further, when such participation is recognized and its importance acknowledged, positive consequence for public confidence in the process might well follow.

There is much more that needs to be said. Discussion of public involvement in the setting of priorities related to science should consider our various attempts to improve the public's understanding of science, the problems of providing effective science education, and the public interest-science movement. Consideration of such matters would require a very lengthy extension of these remarks.

Thank you very much.

[The prepared statement of Dr. Hill follows:]

Testimony before the Subcommittee on Science, Research and Technology and the Subcommittee on Investigations and Oversight

Richard J. Hill

University of Oregon

The process of resource allocation in an open, pluralistic society becomes increasingly critical when available resources diminish. What seems to be most vexing to many of my scientific colleagues is the apparent irrationality of the allocation process. The process is not well-understood. It is not even well-described. Clearly, in the public sector, such decisions are political and are derived from social and economic assumptions that change periodically and are never universally acceptable. Thus, we find ourselves in the totally predictable situation where the process becomes an issue and where those involved in setting priorities become suspect.

It is clearly in my self-interest to urge the Congress to increase support for scientific research and development. There is a multitude of problems within the scientific enterprise that needs to be addressed. In certain areas, the pool of highly qualified scientists and engineers is so inadequate that vicious and disruptive competition for talent occurs. Within research universities that process is not new, but the intensity of the struggle is increasing not only among the universities but between the universities and other institutions both private and public. There also are areas in which our lack of support for instrumentation apparently has led to a decline in our nation's standing within the world's scientific community. For example, the earlier dominance of the United States in accelerator-related atomic and molecular physics has been eroded.[1] This litany of woe could be extended, but I believe such an extension is unnecessary. The remedy for such troubles is dependent upon a general increase in the level of support available for science.

Under existing political and economic conditions, the arguments advanced by the scientific community for increased general levels of federal support probably will not carry the day. Thus, at least in the immediate future, the allocation of existing resources becomes increasingly critical.

If I am correct that the process of allocating public resources is essentially political, then we must expect the assessment of probable social and economic payoff to be an important consideration. That payoff is manifested in the applications of science to the solutions of problems and in the development of new technologies.

I believe that at any moment in the total development of science, we can make very good guesses about those areas of activity likely to have the most immediate payoffs. Judgments of this sort have to do with what has been referred to as the "ripeness" of science.[2] An area of science reaches this stage only when a great deal of work has been accomplished and, perhaps more critically, when the importance of what is not known is recognized. In describing his work on lasers, Nobel Prize laureate Arthur Schawlow said:

> "You have to know something, but what you really need is to recognize one thing that's not known. And once you realize that you're looking for the gaps, it isn't so hard."[3]

As an area of science approaches "ripeness," the gaps in our knowledge are more easily recognized, their importance is more readily assessed, and progress can advance more rapidly.

It seems clear, for example, that molecular and cellular biology have reached the ripe stage. It also is relatively easy to predict that one of the next stages of the high-technology revolution will be in the area of bioengineering. Further, the social and economic consequences of these developments are likely to be of major significance in areas as diverse as medicine and forestry. To the extent that payoff is a consideration, the allocation of resources to these areas is increasingly a low-risk venture.

It is important to recognize that when an area of science reaches this stage of maturity the resource base is likely to change, with the private sector becoming an increasingly important source of support. The developments in microelectronics clearly illustrate the principle. Computer and data processing firms have made generous contributions to university research and educational programs, as well as heavy investments in their own research. As such investments increase, the availability of public funds becomes less crucial for the continued progress of developmental activity. Some public resources will still be required for basic research, but the funding of applied research and development can be assumed by the private sector.

To the degree that relatively immediate social and economic consequences are a concern, judgments about the maturity of an area of scientific inquiry become important. Here I believe that the scientific community is the only segment of our society qualified to perform the task. Scientists, at least some of them, do think and talk about the maturity of their fields. For example, in recent conversation with some of my colleagues, the judgment was made that both the neurosciences and the psycho-biology of cognition were nearing that stage of maturity where important new applications could be expected. If that judgment can be validated, then the investment of public funds to bring these areas of inquiry through that next stage of development is clearly a reasonable decision.

I believe that it is possible to establish a mechanism to provide such assessments of the various areas of scientific inquiry on a continuing basis. Scientists will disagree, for they, too, suffer from the biases introduced by self-interest. Nevertheless, a body like the National Academy of Sciences could assume the function of providing Congress with an ongoing assessment of the ripening process.

Assigning high priority to areas of science nearing maturity will have costs to other areas of scientific activity if the total resources available are not increased. Here we directly confront some very hard decisions. My position on the matter will not be well-received by all of my colleagues, but I am convinced that it is sound. The federal government must continue to be the major source of support for basic research. In disciplines that have not reached the stage of maturity of, for example, the physical chemistry of silicons, short-term payoffs are not apparent and significant support from the private sector should not be expected. The long, tedious, and costly process by which a discipline reaches maturity must not be abandoned. We need to establish a balanced and stable degree of support for basic research across the range of scientific disciplines. We should recognize that until conceptual, factual and technical maturity are achieved in a field, applications are likely to have limited consequences, and such work should occupy a position that is lower on our list of priorities. Support of basic research should be seen as a long-term public investment: an investment that has paid excellent dividends in one area after another as these areas have matured.

The central theme in the above is that in the areas of basic research, scientists themselves should be relied upon to set priorities both within and between areas of inquiry. This process should not operate in a social vacuum. Scientists, by and large, are aware of their obligations to the society that supports them, but this awareness needs to be made explicit and to be constantly reinforced. The responsibilities of the science community deserve extensive analysis and discussion, but I will not consider such matters here.

As we move toward application and technology, the situation changes. It is in its application that the Jekyll and Hyde character of science becomes apparent. A drug that provides tranquility also results in malformed infants. Our society takes for granted the benefits of applied chemical research, but the people of

Baton Rouge, Louisiana, and rural Warren County, North Carolina, are now questioning the costs associated with those benefits. The list of troublesome examples, as members of this Subcommittee well know, could be extended to an appalling length. We have mismanaged the disposal of toxic chemical wastes; the storage of radioactive wastes remains an unsolved social problem; and so on. The public is concerned about these issues, and I have been asked to comment specifically on public involvement in the process of resolution.[4]

The public demand for protection against the Love Canals and the Three Mile Islands will not rapidly dissipate, nor should it. This demand requires that we give high priority to increasing our resources for the general assessment of applied science and technology. We must develop further our ability to monitor, to collect adequate longitudinal data, and to test systematically the by-products of applied science. As Professor Harvey Brooks has stated, the kind of scientific research required is "low-grade, often routine, and scientifically not very challenging,"[5] but it is essential.

It would be difficult to establish such a monitoring system within the current structure of university research.[6] We should not assign the task to the private sector for obvious reasons. The problems involved are not strictly state or local in nature. For example, toxicity and its producers migrate. Thus, monitoring functions, I believe, must be performed within and supported by the federal government. Clearly, some such activity now is underway, and I do not intend to minimize the significance of such agencies as the Office of Technology Assessment, the Bureau of Standards, and the Food and Drug Administration. I do believe that the resources for scientific monitoring need to be increased and better coordinated. Certainly public trust in such activity needs to be improved far beyond current levels.

The question of whether or not the public should be involved in influencing research and development priorities is not really relevant. Important segments of the public are now involved, and in future discussions of the matter, this should be taken as a given. Kenneth Prewitt's recent discussion entitled "The Public and Science Policy"[7] is suggestive. Prewitt treats the notion of specialized and attentive publics. These publics are self-selective, well-informed and organized. Some of these publics emerge as single-issue forces that agitate against various science and technology developments, but there is also evidence of an attentive science public that is generally sympathetic toward science and vocal in its support. My recent experiences associated with making university budget reductions are illustrative. We announced that we were considering closing the University's

Institute of Marine Biology. Within an hour of the announcement, protests were being received. Over the course of a month hundreds of letters arrived from scientists, students, environmentalists, the tourist industry, and members of the state legislature. Interested parties demanded to see the University's President. Among the charges was the common assertion that in these times we needed more science, not less. The fact that an attentive public was concerned about university priorities was documented, and the political nature of the allocation process was again demonstrated: the Institute of Marine Biology remains in operation.

Much of the public's involvement to date has had the character of protest, frequently leading to public hearings and, at least occasionally, to litigation. If we live in a period of technological revolution, we also live in a litigious age; and if our pluralistic society is to be governed by law, we should expect this litigious age to be a long one. The costs involved in existing advocacy procedures bring me back to the monitoring process discussed above. Effective scientific monitoring could expedite review and facilitate decision making, if the monitoring process gained public confidence. Thus, those who would argue that such increased monitoring would constitute costly overregulation should compare the potential of such a system against the costs of current regulatory policies. Consider but a single example. The data accumulated by continuous monitoring activity could provide information that could facilitate the preparation of environmental impact statements and expedite their review.

I have argued that if such monitoring systems are to be effective, they must gain the confidence of the public. Brooks' discussion of such systems includes an important suggestion.

> "Any monitoring or data collection system should be subject to a continuing process of review, assessment, and possible revision by outside advisory groups including scientists, laymen and policy-makers. The review should be much broader in its disciplinary representation than the customary peer review panel or research advisory committee to an agency."[8]

I agree. We need to monitor the monitoring systems and in this task, I am convinced that members of the attentive public can participate in a meaningful and constructive fashion. Further, when such participation is recognized and its importance acknowledged, positive consequences for public confidence in the process might well follow.

There is more that needs to be said. Discussion of public involvement in the setting of priorities related to science should consider our various attempts to improve the public's understanding of science, the problems of providing effective science education, and the public-interest-science movement. Consideration of such matters would require a very lengthy extension of these remarks.

1. See W. E. Meyerhof (editor) Report on the Workshop on Accelerator-Based Atomic and Molecular Science. University of Connecticut, Storrs, 1981.
2. A. M. Weinberg, "Criteria for Scientific Choice," Minerva, Winter 1963, and "Criteria for Scientific Choice II: The Two Cultures," Minerva, Autumn 1964.
3. "Going for the Gaps: An Interview with Arthur Schawlow," The Stanford Magazine, Fall 1982.
4. My current thinking about this set of issues has been influenced by the recent writings of Professor Harvey Brooks. See for example: H. Brooks, "Science Indicators and Science Priorities," Science, Technology, and Human Values, Winter 1982.
5. Ibid. p. 21.
6. The model provided by the Agricultural Extension Service might be extended to cover scientific monitoring, and such a service could be university based. However, developing such a system would be extremely costly and would face serious problems of coordination.
7. K. Prewitt, "The Public and Science Policy," Science, Technology, and Human Values, Spring 1982. For a somewhat different position see D. Yankelovich, "Changing Public Attitudes to Science and the Quality of Life: Edited Excerpts from a Seminar," Science, Technology, and Human Values, Spring 1982.
9. Brooks, op. cit., pp. 21-22.

Mr. Brown. Thank you, Dr. Hill.

Each of you gentlemen has presented papers which are very provocative and stimulating, and we appreciate them very much.

I am hopeful that one of the results of this hearing can be sort of a distillation of some of these key ideas which can be made a basis for more widespread discussion.

I think all of you have indicated and are aware of the need for broader public understanding and participation in making some of the judgments involved in this priority-setting process. There is no magic solution to any of it.

Dr. Hill, you had a section of your statement that made a rather interesting comparison between science at a stage when it is developing and at a stage when it is more mature, and it led me to thinking at the time, looking at the program of the National Science Foundation, that we have a situation where there has obviously been a sharp change in policies of support for science in which the more mature sciences are getting a substantial increase and the less mature—and I am thinking specifically here of things like the social sciences and so forth—are being relatively neglected.

I would like to have you comment on that in the light of your statement, how you see that as a proper exercise in priorities, or would you have criticism of it?

Dr. HILL. Well, I have some biases. I am a social psychologist. I believe that the pressure for public accountability of science and the political nature of the allocation process has led to certain emphases on payoffs. I believe that is appropriate for the total society, but at the same time I think you have to stabilize the support for basic research across the range of disciplines. I do not believe that is a contradictory position. I understand the shift in support for the mature sciences and the assessment of the probability of those sciences contributing to broader social problems. I am very much influenced by such things as the 5-year report from the National Science Foundation and the annual reports that they provide, and I believe that is a tremendous service to us in making decisions about priorities.

Mr. BROWN. Well, I was going to raise that. You mentioned the 5-year outlook. The reason those were included as a specific component of the act setting up the Presidential Science Adviser was as an aid in this priority-setting process, and I gather from your comments that you feel that this is a useful tool in allowing judgments to be made across scientific boundaries with regard to relative state of maturity and state of opportunities for further development in these sciences; am I correct?

Dr. HILL. Yes, sir.

Mr. BROWN. You have discussed at some length the general role of monitoring functions, and I am still not clear in my own mind how to best incorporate this particular aspect of science into the overall structure of support for science, and I have wrestled with it for a number of years. It seems that support of this kind of science is going to be criticized from various quarters, but for example within the scientific community on the grounds that you have enunciated here that it is really not that exciting a science.

Dr. HILL. True.

Mr. BROWN. It is a science which may not attract the most brilliant minds in a particular field, and yet it is essential for social purposes. Is there any other grounds that we can use to justify the support of it other than the importance of its role in well, say, improving the quality of life for society? Can we tie it in with the matter of public education in science or with the element that you are quite familiar with, the improvement of public participation in science, and bring about a stronger base of justification for it in that way?

Dr. HILL. I believe you can. I think the scientific community has to recognize that until we have better public understanding of science, stronger science education, that the public is not going to make sharp distinctions between the science as a basic inquiry into nature and the byproducts of science which are occasionally very costly to populations. Science is seen as a lump of activities, and it is in the best interest of the scientific community, no matter how pure and pristine a particular scientist is, to be concerned about the public's perception of the costs of science as well as their benefits, and the costs of radioactive waste leakage, toxic waste problems, the possibility of climatic change as certain fossil fuels are consumed in greater degrees, and so on—these are things that the public, the informed public does know about and is concerned with, and if the scientific community does not respond to those kinds of problems, the scientific community itself will in the long run I believe lose the kind of support that we are so dependent on. It is a matter of self-interested responsibility on the part of science.

Mr. BROWN. Do you have any questions?

Mr. WALGREN. I apologize for being absent, and perhaps this subject has been covered, but is there a way that you could make an estimate of what the optimum level of investment in science, either field by field or overall, might be? We always think we are underfunding, and these days there is the rush to cut Government budgets just to cut Government budgets, regardless of what they are. Are there scientific models that could tell us whether our society is overinvesting or underinvesting for an expected yield?

Dr. ROTTENBERG. There is a model to determine how resources ought to be allocated to alternative uses. Economic activities ought to be of relative sizes such that the return on investment in all activities is normal. The problem is that in the case of activities like scientific research, there are many variables that are difficult to measure explicitly. If there is a group of researchers in the field of biochemistry who are discovering something new about molecular cells, it is difficult to know what value to put on an increment of pure knowledge of that kind. It is difficult to know how long that increment of knowledge is going to survive before it becomes intellectually obsolescent, before another team of biochemists discovers something which makes the prior knowledge untrue. It is difficult to measure the magnitudes of the variables that are relevant to the generation of the rate-of-return number. Therefore, it is difficult to know whether wrong sizes or right sizes are being generated. In principle we know what it should be. It should yield a rate of return on investment which is equal to the return on investment in other lines. Empirically measurement is what is difficult.

[The material follows:]

It is for that reason that I have suggested that, in the case of applied research, the scale of investment in diverse fields and projects should be determined by the market, rather than by governmental officeholders who plan the allocation of research funds. The market tends to casue socially optimal scales of investment to occur. And it is for the same reason that I suggest that, in the case of basic research, government agencies engaged in funding research should avoid the rank-ordering of fields and projects but should, instead, attempt to seek out scientists of intellectual achievement and promise who would be funded for the work they do.

Dr. WEINBERG. The question you raised of course is almost the central question in science policy and one of the main issues discussed in this book that was published 15 years ago under the auspices of this committee, "Basic Research and National Goals." The view that many of us took in putting together those considerations at that time I think may be relevant today. As I said in my testimony, we argued that the concept of science as a whole, which is perhaps implicit a little bit in the way you phrased the question, how much money should we be spending on science, was really an improper way of setting the issue; that much science—in fact, most of the money the Federal Government spends for science is not spent for the most basic kind of activities that have no relevance to anything, but instead are spent by the mission-oriented agencies as a means of accomplishing their mission.

Now, we do not particularly ask—I do not know—we do not ask NASA, how much money should you, Mr. Administrator of NASA, spend on transport in achieving the mission of NASA? We do not ask them how much do you spend on accounting to achieve the mission of NASA? And we argued in basic research and national goals that, in the same spirit, applied science ought to be viewed as an overhead expense to be tied to the mission that the science was supposed to accomplish.

Now, overhead expenses are always the most difficult ones for a company to decide on. How does a corporation decide on how much money it ought to spend on advertising? Well, it tries to use the signals from the marketplace to decide whether it is spending too much or too little, but the signals are not very precise, and therefore it finally comes to somebody else's judgment. The opinion we had here was that it was best to allow those judgments to devolve on the people who were responsible for accomplishing the mission.

There remains the question of what do you do about the general residuum of science, which I have called intrinsic basic research. I try to argue that that should be regarded as an overhead expense against the entire scientific and technical enterprise, which included not the manufacture of materials, but rather the total applied scientific and development activity. At the time basic research and national goals was written, this was a rather small fraction of the applied scientific and technical enterprise.

As was pointed out by Dr. Rottenberg, there are difficulties in so viewing basic research; at least in the case of the small-scale basic research, it is probably correct that if you let the scientists, governed as they are by the interaction between scientists and between competing scientific doctrines decide, they will by and large come out with a good allocation. However, they will not come out with a decision as to how much the entire scientific activity ought to command. Indeed Harry Johnson, this famous economist that we scientists had so much trouble with 15 years ago, argued that that was sort of self-defeating, because if you allow even the best scientists to say how much we are going to do here and we say we are going to support them, then you have built in a very strong attractor, and people will start moving into those fields, and so that field will start getting larger and larger, people will find jobs, find ways of making a living as well as a way of life in that kind of science, so that that by itself is not sufficient. I guess that is the main objection that I would have to too complete a dependence on the intellectual marketplace for establishing the levels of scientific activity.

Mr. WALGREN. Thank you very much.

Mr. BROWN. Well, let us continue this discussion for a minute. I think you gentlemen all suffer from the sin of overrationality more than anything else. The problem of setting priorities in science could be left to scientists in selecting the best scientists, but take the national health field. We are devoting a great deal of money to research in the causes of disease. We are getting into basic biology to try and understand cancer and so forth. Most of the diseases that we have solved have not been solved that way. They have been solved by attacking some other aspect of say the carrier or something of that sort without knowing too much about the basic mechanism.

Suppose the attack on the basic mechanism is fundamentally wrong and we should be devoting ourselves to some other area. What mechanism in science would bring about that kind of a direction, or to look at it even more broadly but still in the health field, suppose the real problem is we live in an unhealthy society, and that all of the factors of this society are stress-producing, and that we really need to devote more effort to healthy lifestyles and a healthy social environment rather than we do to the basic causes of disease. Where in the medical profession are you going to get that sense of priorities reflected?

I do not quite see that, and it leads to the idea that there needs to be levels of priority-setting in which the fundamental decisions basically move up I guess through a political ladder, and we hope that we get more reasonable politicians in the long run. You could use this sort of an analysis for energy, as you well know. Perhaps the whole focus of energy research, which basically now and over the long run has been enhancing energy supply, conceivably could be wrong. Perhaps we ought to design a society which uses the least amount of energy and solve our problems that way. But there is very little in the system that allows us to look at the problems from that sort of a standpoint. The vested interests are always contrary to taking that approach which rises above the current situation.

Dr. ROTTENBERG. I think it is true that professionals will be specialized in their perception of problems because there are disciplinary constraints which their studies have imposed on them. Biologists and chemists will look at disease problems in ways that are outgrowths of their training. But the scientific community is not composed just of those. It includes also social psychologists and epidemiologists.

Mr. BROWN. But they are not getting any funding these days?

Dr. HILL. We are getting some funding for epidemiological research.

Dr. ROTTENBERG. Whether they are funded by Federal Government agencies or not, they are engaged in research. That is to say, the universities fund them by requiring them to appear before classrooms on the order of 6 hours a week. The rest of the time is theirs to do scholarly work. So even without Federal funding, some are engaged in research. If, in fact, the stress is the progenitor of disease, it will be discovered, because the sense of the scientific community will cause those researchers to search out fruitful results.

Dr. WEINBERG. I guess I would simply point out that we must be doing something that is very right if we look at the health of the American society. Our life expectancy has increased a couple of years in the past decade or so. One of the things that I would perhaps take issue with my distinguished colleague from the University of Oregon is in his stress on a greater concern with the environment, the implication I suppose being that well, if you clean up all these things in the environment, then we are going to live longer and live better, but we are already living longer and we are living better. Some people speak about a cancer epidemic. There is no cancer epidemic. We are living longer. We have so much improved the survival against death from cardiovascular disease that many of us are becoming prey to cancer, though the fact is that the age-adjusted total cancer mortality has hardly changed.

It is true that lung cancer has gone up because of cigarettes, everybody believes, but stomach and colon cancer have gone way down. There is a recent report from the Framingham study (Journal of American Medical Association)[1] in which the question is asked "Could smoking be good for us as far as cancer of the stomach and colon are concerned?" If, for example, as some of my colleagues believe, people who have a predisposition for cancer, if they smoke too much, will be taken by lung and therefore will not be eligible for being taken by stomach and colon. It is not a trivial idea, and there may be something to it.

I would like to get into this colloquy with Dr. Wyngaarden, because this is not the official view of the NIH. With respect to how these priorities get set, they ultimately of course are political decisions. One of the biggest decisions that was made in the health field as you remember, Chairman Brown, was the decision to solve cancer, Roger R. Williams et al., so we established this national plan and we are up to a billion dollars a year to try to solve cancer. The fact of the matter is that we are living longer, which means that somehow we are doing something right.

When all is said and done, apparently richer is healthier and richer is safer. The richer we become the longer we live.

We kind of forget that we are all mortal, no matter how medical science advances, the probability for dying for everybody in the room is unity.

Mr. **Brown**. Let me ask one additional question. What is the effect of a period of high inflation and high interest rates on the funding—on the economics of funding basic research, which has no short-term economic benefit?

Dr. **Hill**. In certain areas it drastically reduces the usable resources available. I think you see this very well illustrated in certain problems of instrumentation, where because the basic research budget has not kept up with the increase in cost due to inflation, our ability to keep at the state of the art——

Mr. **Brown**. You are mentioning the practical aspects of it. I want Dr. Rottenberg to tell me the theoretical aspects.

Dr. **Rottenberg**. I think it may be a false premise that basic research produces fruitful results only in the long run—I think that may be a false premise, because it assumes that the value to society of basic research is the fallout of practical applications that occurs in the long run. If it is perceived that the product of basic research is not the fallout of practical uses, but rather the increments to pure knowledge which is generated by the research they may be as short run in being realized as the results from applied research. If you think of basic research as adding increments to the stock of pure knowledge, it may be that we must wait for them for no longer an interval than the period in which we must wait for the results of applied research.

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[1] Cancer Incidence by Levals of Cholesterol," Journal of the American Medical Association 245 (3): 247-252, Jan. 16, 1981.

Mr. **Brown**. Can you quantify that economically, the benefits of additions to pure knowledge?

Dr. **Rottenberg**. No; what I said in my statement was I cannot, and that is the big problem. I do not know what the comparative values to society are of increments of astronomic pure knowledge and increments of knowledge about the physical qualities of metals, and so on. If it has no applied use but is simply an increment of pure knowledge, it is difficult to measure its value or it may be impossible to measure its value, and that is precisely why I have difficulties in suggesting what the appropriate scale for society is of basic research and, within that the magnitude, of Government support for basic research.

Now, let us go back for a moment—suppose you perceive that fruitful results of basic research occur only when applied uses fall out in the end, and suppose it takes a long time for that to happen then the higher the interest rates, the less valuable in the present are those fallen-out results distant in the future. High interest rates imply in theory that it is less worthwhile for society to wait for the fruits of its investment and, therefore, they imply less support for basic research than do low interest rates.

We must be careful, however, in the "reading" of nominal interest rates that we observe in the marketplace. Nominal interest rates fluctuate from day to day and interest rates differ, one from another, depending upon the uses to which loan funds are to be put, the creditworthiness of borrowers, the nature of collateral, and other variables.

Nominal interest rates are not relevant to decisions about the support of basic research. Nominal interest rates should be decomposed into the component that compensates for expected increases in prices over time, the component that compensates for risk, and the component that compensates for the "painfulness" of waiting. It is only that last componant that is relevant to the basic research support decision. That component—the payment for waiting—is, of course, much more stable over time than are the nominal interest rates observed in markets.

If the part of interest rates that pays for waiting does not rise from one period to another, the proper inference to be drawn is that support for basic research should not be diminished, even if marketplace interest rates do rise.

That is consistent with what you expected, I take it.

Mr. **Brown**. Yes, quite consistent.

Thank you very much, gentlemen. We appreciate your testimony, and we will look forward to hearing from you again.

I am going to go back and read Polanyie.

Dr. **Weinberg**. So shall we.

Mr. **Brown**. Last, we have two distinguished representatives of State government to help us out in this question. I would like to invite both Dr. Lindsey and Dr. Casteen to come forward.

Dr. Lindsey is science adviser to the Governor of North Carolina and Dr. Casteen is science adviser to the Governor of the Commonwealth of Virginia.

Would you proceed, Dr. Lindsey?

[The biographical sketch of Dr. Lindsey follows:]

Dr. Quentin W. Lindsey

Background in analysis of the structural organization of societies, including the design of institutions and the formation of policies and programs essential to development and change. Experience includes (1) principal investigator for a number of research projects while with the Research Triangle Institute; (2) assisting the Government of Egypt in planning the use of Egyptian scientific resources in the development of Egypt (3) study of the Research Applied to National Needs programs of the National Science Foundation; (4) numerous short assignments in Asia and Africa for the Ford Foundation, U.S. AID and the United Nations; (5) advisor to the Government of Nepal (six years); (6) advisor to the Government of Burma (one year); and (7) graduate teaching and research (eight years) relating to the economic, social, political and philosophic foundations of national policy.

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1968-69, visting professor, Carolina Population Center and Department of Economics, University of North Carolina.

1962-68, economic advisor and Project Leader, His Majesty's Government of Nepal (Ford Foundation).

1961-62, economic advisor (Ford Foundation), Union of Burma.

1955-61, associate professor, Department of Economics, North Carolina State University, Raleigh, North Carolina.

1951-54, project leader, Southeast Research Committee (An association of Economics Departments of southeastern universities).

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STATEMENTS OF DR. QUENTIN W. LINDSEY, SCIENCE ADVISER TO THE GOVERNOR, STATE OF NORTH CAROLINA

Dr. **Lindsey**. Mr. Chairman and subcommittee members, I thank you for the privilege of appearing before you. My written testimony, which I provided in advance, is too long to present verbally.

Mr. **Brown**. It will be in full in the record.

Dr. **Lindsey**. Therefore, I will summarize that testimony now and add a few comments and respond to questions if you have any.

In view of the questions you have been raising with others giving testimony, I should inject here that in making decisions with respect to where our resources ought to go relating to scientific research, my preoccupation has been with the balance between basic research and those processes by which we insure that society utilizes and benefits from research.

The thesis of my testimony is that the center of gravity for technological innovation in the United States must shift from the Federal level to the State level. This thesis, and associated reasoning, is taken from an address by Governor Hunt at the annual meeting of the American Association for the Advancement of Science, last January.

Technological innovation, in this context, refers to the use of basic scientific research for creative, peaceful purposes, within the United States and throughout the world. It does not include use of science to further military purposes or space exploration.

Such a shift in leadership from the Federal to the State level requires a working partnership of State and local governments, industrial workers and management, research and educational institutions, and people. This partnership is feasible at the State level, provided principles of integrity, justice and mutual concern for the well-being of all people guide the working relationships.

It is not so feasible at the Federal level, given our antitrust legislation and the processes by which influence is brought to bear upon government: large, sophisticated organizations find it is to their advantage to lobby a large central government; it is more difficult to influence 50 States and hundreds of local governments. Conversely, numerous small and less sophisticated firms and organizations, and individual citizens, have difficulty in influencing a large central government; but for common interests they have easy access to State and local governments.

By technological innovation, I mean those technical and organizational changes that result in new and better products, in modification of human relationships, and in the retraining of people for different roles in society as robots and other sophisticated methods become possible through scientific exploration.

Japan and other nations are surpassing us in perfecting these innovative processes. Given the size and nature of our society, I do not believe that it is possible for the working partnership of which I speak to be administered or otherwise achieved when the primary thrust is centered in Federal agencies.

For military and space purposes, DOD and NASA initiate innovative development and new weapon systems and space shuttles—and are also the customers that acquire and utilize these new products. Federal leadership in this case is essential. But for the larger and more complex aspects of private sector consumption and international markets, the working partnership must be decentralized to the State level.

The scope of activity of which I speak must range from improvements in elementary and secondary education, particularly in science and mathematics, through the structure and functions of research and higher education, and encompassing the institutional relationships and operating procedures comprising the innovation process itself.

By the innovative process, I refer to venture capital institutions, incubator conditions whereby innovative concepts stemming from basic research are brought to fruition, and other activities essential to effective utilization of research results.

I contend that the requirements of technological innovation for peaceful purposes are of sufficient magnitude and complexity to justify creating a special task force at the Federal level to explore the process carefully and to recommend essential policies and procedures, including structural changes that may be necessary at both Federal and State levels.

Governor Hunt has suggested that something analogous to the Morrill Act, which created the land grant college system, might be the result. The implication is not that a new set of institutions needs to be created. On the contrary, it is that a redefinition and clarification of Federal-State relationships is essential.

In reviewing the material that your staff provided as background to this hearing, I was disappionted to find that neither Dr. Keyworth nor Dr. Press nor Dr. Stever saw fit to consider the potential role of States in the pursuits that I have emphasized.

I have very high regard for these gentlemen and attribute this omission to, first, the fact that none has had experience in operating at the State level and second, that physical scientists who are preoccupied with basic research are not accustomed to dealing with the organizational structure of society nor with the processes by which the results of basic research are utilized. My testimony is intended to be a step toward filing the gap that Presidential science advisors tend to overlook.

Finally, I feel compelled to close with a personal observation as to the importance of the approach that I urge Congress to consider. The responsibility for this observation is mine alone, although I suspect the issue to which it relates troubles us all.

The point is this: Utilization of the power of science in the United States and in the U.S.S.R. is now dominated by military purposes. Nearly $24 billion of the $42 billion Federal outlay for research and development flows through DOD. In the words of a philosopner friend of mine at the University of North Carolina, this is madness; particularly uses directed toward the perfection of nuclear weaponry.

Madness is used here in a way analogous to use in the movie "Bridge on the River Kwai." There, you may recall, a career army colonel and his troops, as prisoners of war, constructed a railway bridge in the jungle of South Asia. The bridge was for use by their captors and they were driven to build it by their scientific and engineering skills, together with their organization and discipline, forgetting that it would enhance the destructive capabilities of their captors.

Upon completion, the bridge was blown up by a special contingent of troops who came to destroy the bridge and perhaps rescue the colonel and his soldiers. But in destroying the bridge they also killed the colonel and many others, and were aghast to learn that it was the colonel and their own troops that built the bridge, not the enemy. A member of the rescue contingent, upon realizing the full import of the terrible scene of destruction, termed it all sheer madness.

But who was mad? The colonel? The rescue team? Or were we all mad, friend and foe alike, to have created a situation that could give rise to such carnage?

In this scene on the River Kwai, the destructive capacity of human beings was at once caused by, and limited to, the power of science as demonstrated by the engineering feat of building the bridge and the power of the explosives that destroyed it. Heretofore, in the history of civilization, the destructive capability of humanity has always been limited by the limits of our own scientific knowledge.

Now, however, the limits of this knowledge, particularly as manifested in the perfection of nuclear weaponry, have pressed our destructive capability beyond the limits of civilization. We can now destroy ourselves and the civilization we have created. The limits to scientific knowledge will no longer protect us.

Underlying my emphasis upon the use of the power of science for creative purposes, for perfecting the process of technological innovation for the benefit of this and other societies, is the conviction that we will not move away from military dominance until we can visualize clearly a better alternative; that is, until the potential of military influence shrinks in proportion to the potential of other measures.

Implicit in this view is the proposition that there are substitutes for military force in dealing with human affairs. To put it very simply, in my view, the slogan, "the best defense is a good offense" has relevance here.

The Department of Defense outlays for R&D are by definition defensive. A strong military offense in world affairs is reprehensible. But deliberate use of the power of scientific knowledge for the creative purposes I have suggested in both domestic and international pursuits is an offense that is both morally and politically acceptable.

Thank you.

[The prepared statement of Dr. Lindsey follows:]

TESTIMONY OF DR. QUENTIN W. LINDSEY
SCIENCE ADVISOR TO THE GOVERNOR OF NORTH CAROLINA
to the
SCIENCE, TECHNOLOGY AND RESEARCH SUBCOMMITTEE AND THE
OVERSIGHT SUBCOMMITTEE OF THE HOUSE COMMITTEE ON SCIENCE & TECHNOLOGY
SEPTEMBER 30, 1982

Introduction

Mr. Chairman, I am Quentin W. Lindsey, Science and Public Policy Advisor to Governor James B. Hunt, Jr. of North Carolina. I am pleased to be able to appear before you today.

The issues which you have chosen to address during these hearings are of great concern to us in North Carolina. Governor Hunt, as Chairman of the National Governors' Association Task Force on Science and Technology, and Chairman of the Education Commission of the States, also has a keen interest in science and engineering R&D priorities across the country. I am fortunate in having worked with other governors and their science advisors as we strive collectively to establish an agenda for cooperative ventures into science and technology in the 1980's and beyond. Of course, we have taken several steps in an effort to move our own state into the forefront of technological development.

I commend members of these subcommittees for the important steps you are taking to explore the actions necessary to create a national consensus for setting priorities in science and technology research and development.

Underlying Principles[1]

The ability to transform advances in scientific knowledge into new or improved products and services underlies virtually every improvement in economic productivity and social well-being. For years during and following World War II, the U.S. led all nations in utilizing scientific knowledge in developing new products or improving those in existence. Now, we are being surpassed by other nations and a serious crisis is emerging: Basic research accomplishments no longer percolate through the system with sufficient rapidity and effectiveness. Whereas U.S. Government expenditures for R&D are impressive, the extent to which the results of research are utilized by state and local governments, by federal agencies and by private industry for the benefit of society is disappointing. Japan, for example, drawing upon basic research results generated in the U.S., is exceeding our ability to transform fundamental knowledge into useful products with worldwide market potential. Concerning human health, to cite another illustration, more scientists in the

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[1] This section is derived from "Academia, Industry and Government: The Organizational Frontier of Science Today," an address by Governor James B. Hunt, Jr., at the Annual Meeting of The American Association for the Advancement of Science, Washington, D.C., January 4, 1982. A copy is attached.

U.S. have received Nobel awards than in any other country in the world, many of whom are in health-related disciplines. Yet our citizens are no longer judged to be the "healthiest" by standard measures of health, and the cost of health services is becoming seriously inflated.

Economic productivity is determined by several factors, one of which consists of the results of basic research. The fact that our productivity has leveled off and even declined somewhat cannot be attributed to a void in fundamental knowledge. It is more properly ascribed to our failure to make effective use of what we do know.

The present situation presents a challenge to our ability to innovate comparable to that which we faced in World War II. The overriding question now, as then, is how to organize our remarkable scientific, engineering and technical capability. Now it should be for peace; then it was for war.

The situation both then and now demonstrates the central importance of science and technology to this or any society. It follows, therefore, that those who devise the organizational means to resolve this emerging crisis--who successfully redefine the role of government in relation to research and education, to industry and to the general public--will also reap the rewards of economic and political support inherent in the fundamental desires of Americans throughout this country. The importance of technological innovation arises from the fact that no modern society can function as such today without making effective use of scientific knowledge. Our military strength depends upon it. But of greater significance to the emerging crisis today is the dependence of economic productivity upon our ability to innovate, both technically and organizationally. The problem is that we still have essentially the same organizational structure for science and technology that was designed in World War II and the Korean War. Yet we need a structure more in accord with conditions of today.

Federal expenditures now dominate all research and development in the U.S.--$42 billion out of a total of more than $70 billion. Within the federal outlays, defense-related expenditures are by far the most dominant category--$24 billion out of $42 billion. Thus, in several respects, it may be said that science and technology have ridden the coattails of defense and space since World War II.

The unique aspect of military and space research is that provision is made for technological innovation by actually producing and using weapons systems, rockets, tanks, ships and guns. DOD and NASA organize to use the results of research in fulfilling federal objectives. Whereas, for fundamental research, little deliberate provision is made for effective nonmilitary utilization of research results--that is, for those organizational arrangements analogous to the processes by which the Japanese have developed such a strong international market position. This void in our ability to utilize effectively and for peaceful purposes the results of our basic research is the critical fault of our present system.

I am convinced that there are alternatives to the armament race and that they lie in forms of technological innovation directed toward the needs and concerns of local people in every nation. We have demonstrated, through our World War II and subsequent experiences that it is possible to harness science for unprecedented warfare. But it is also within our power to design a world society capable of striking an ecological balance between population and environment consistent with such guiding principles as justice, integrity and compassion.

State Government Responsibilities

Before I turn to the responsibilities of the federal government, let me focus on the state and local level for a moment. The premise upon which we base our efforts in North Carolina is that, even with a strong federal leadership role, the states must develop a distinct and significant role in overcoming the deficiencies of the present structure by strongly and deliberately fostering technological innovation for the benefit of society.

An effective organizational arrangement at the state level is essential. It must include three critically important components under the leadership of the Governor: (1) state and local government leaders; (2) labor and management leadership from industry and agriculture; and (3) leadership of research institutions, particularly universities. Legislative involvement and support is also necessary, as well as understanding and cooperation from the general public. Contacts with appropriate federal agencies should be developed consistent with the orientation of the state program.

No formal pattern is proposed here today. Each state must analyze its situation and take action accordingly. The function of the organization is to ensure that policies of critical importance to the state, and that require the use of scientific and technological resources in their formation and implementation, are indeed decided and carried out with appropriate scientific input. This will entail both significant financial support to R&D within the state and provision of appropriate guidance to such activities. The benign relationships of the past, coupled with weak or non-existent financial support, cannot continue.

The role suggested should not be construed as an infringement upon the prerogatives of the universities, nor as a means of insisting that all scientific research should be goal or policy oriented. Fundamental exploration of science can and must continue, and with much stronger financial support from states. But, in addition, it is the responsibility of state government to ensure that the results of scientific exploration are used for the benefit of society. Moreover, it is contended that the necessary financial support from state governments will not be possible without demonstrating that significant state benefits will accrue as a consequence.

Among the steps set forth by the National Governors' Association (NGA), two illustrate what states can do. The first is to extend and enhance the training of scientists, engineers and technical personnel in fields in which such improvements are needed. The second is to ensure that workers have a stake in innovation processes and are directly involved in decision making.

Taking the long view, improvements in training must begin at the elementary and secondary education levels. Several states have introduced measures intended to improve the proficiency of students in reading, writing and arithmetic in the early years of schooling. Some states are establishing special programs to improve science and mathematics instruction in secondary schools.[2] The American Association for the Advancement of Science (AAAS) is involved in a multimillion dollar program with similar objectives: to improve science and mathematics instruction in secondary schools throughout the nation. We believe state governments can assist in such endeavors.

Enough experience has been gained in recent years for any state to begin educational reforms in both elementary and secondary schools. NGA should continue to serve as a means of exchanging information regarding these experiences. In many states, however, strong, innovative action is needed, along with vigorous legislative support.

Turning to higher education, NGA has set forth several policy options from which any state can select in overcoming critical shortages of engineers, scientists skilled in biotechnology, technicians and so on. Each state will need to examine its particular needs and deficiencies, however, and deliberately plan corrective action. Significant technological innovation is dependent upon overcoming these deficiencies. Legislative and industrial support will be necessary. Leadership by the Governor in targeting action and gaining support is indispensable.

Enabling workers to have a stake in innovation will require imaginative action by any state. The historic adversary relationships between management and labor will tend to inhibit attainment of this objective. David Kearns, President and Chief Operating Officer of Xerox Corporation, put the issue in sharp perspective at the NGA meeting in 1981: "Innovation will not take hold until management understands that employees have a great deal to contribute. The most important resource that we have is our people."

Involving workers in innovation results in more than higher productivity and receipt of materialistic benefits. For workers to participate in decision-making and to know that they are vitally essential often means more than materialistic rewards.

The first step in any state, it would seem, is for selected labor and management leaders to meet together to determine mutually advantageous steps to be taken. A sustained effort will be needed. State government can foster this dialogue through the organizational arrangement noted above. It can also select appropriate policy options to assist workers in adjusting to the employment effects of technological innovation and to assist both management and labor to survive cyclical fluctuations in the economy.

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[2] Louisiana, New York and perhaps other states are giving serious consideration to establishing much stronger programs. North Carolina has established the N.C. School of Science and Mathematics and is taking further steps to improve the proficiency of students in these subjects throughout all elementary and secondary schools in the state. See *The North Carolina School of Science and Mathematics: A strategy for Improving Education in Science and Mathematics* (Raleigh: Office of the Governor, 1980), and *Improving the Quality of Science and Mathematics Instruction in North Carolina's Schools* (Raleigh: N.C. Board of Science and Technology, 1981). Also of interest is (1) the report to President Carter: *Science and Engineering in the 1980s and Beyond* (Washington: National Science Foundation and U.S. Dept. of Education, 1980); and (2) the Carnegie Foundation report, *Giving Youth a Better Chance: Options for Education, Work & Service* (Washington: Jossey-Bass, 1979).

Linking R&D with technical innovation is of critical importance in stimulating innovation. So is the provision of venture capital and the promotion of resource use efficiency, including efficient use from an environmental management standpoint.

Among the policy options for the R&D linkage are tax credits to encourage funding by industry, and matching grants whereby private contributions to universities are matched by public funds for specific purposes. The training of scientists and engineers should occur in close association with the research. Research assistantships and post-doctoral programs are illustrative methods by which this is done.

Provision of venture capital requires bold, experienced business leadership, links to research institutions and venture capital markets, assurance of effective managerial training for new entrepreneurs, and methods of evaluating the technical and economic feasibility of proposed ventures. All these and more comprise the "package" of activity essential to successful technological innovation. Most states have some semblance of all these components. What is lacking is a strong, effective focus and assurance of a level of effort to succeed with risky but highly significant technological breakthroughs.

Resource use efficiency is essential in the short-run to ensure competitive production costs. But short-run conditions must be related to the potential quantity and quality of resources available in the future. Technological innovation can greatly affect the nature of resource use, the volume and characteristics of waste accumulated, and the beauty and ecological viability of the environment in general.

The effects of meeting educational needs and stimulating innovation manifest themselves in increased productivity. Economically, this is measured by increased output per worker, by lower costs per unit of production, by larger gross national product, by a reduced rate of inflation and so on. Other indicators include a scientifically stable environment of high quality, and a citizenry convinced that economic, social and political justice prevails.

Whereas investing in people and stimulating innovation establish conditions essential to increased productivity, incentives to do so must exist as well. For entrepreneurs, this means that a stable and predictable economic environment must prevail, including interest rates and tax policies conducive to investment, and regulatory measures designed to induce conformance with essential rules rather than opposition. For workers, incentives mean participation and security, and a desire to excel with quality performance because there is a direct relationship between performance and tangible and intangible benefits.

Economic conditions designed to provide strong productivity incentives depend heavily upon national policies. Nevertheless, states also exercise considerable positive or negative influence through tax policies, environmental regulations, labor laws and other measures. Critical review of conditions within the control of states is therefore an essential part of the program.

Local Government Responsibilities

In making local governments an explicit part of the state partnership, it becomes possible for them to relate effectively to local industrial firms and financial institutions. It also provides improved linkage with research institutions within the state, and a better understanding by local governments of the utility of scientific research in relation to their own concerns and operating requirements. Local governments must be directly involved in many of the decisions relating to water and sewer facilities, housing and transportation needs, etc., pertaining to industrial location and growth.

An important proviso at the local level is that local governments must establish organizational means by which partnership relations can be pursued. Large cities may establish units analogous to that suggested for the state level. But small metropolitan units and counties may find that county leadership, with metropolitan units participating, is more effective. In still others a regional council of governments may be preferred as the unit most directly concerned with utilization of scientific knowledge.

The Federal Level

Now let me turn to federal responsibilities. I propose, Mr. Chairman, that the Congress and the President jointly create a special panel or Task Force to establish a national agenda for scientific and technological innovation. Representatives from all levels of government, industry (management and labor), the scientific community (scientists and administrators), and the general public should be seated on this panel. The maximum number should be 25, and illustrative representation is as follows:

--Science Advisor to the President

--Representatives from NSF and OMB

--Representatives from research components of federal departments and agencies

--Governors from the National Governors' Association

--Representatives from local government associations

--Representatives from AAAS, NAS and NES

--Representatives from industry

--Representatives from the general public

The purpose of this representative group would be to serve, for a finite period of 12 to 18 months, as the deliberative body responsible for defining federal goals and objectives relating to science and technology policy, particularly with respect to state leadership in utilizing the results of research in the interest of society through technological innovation. Basic or non-goal oriented research, such as funded by NSF and other departments and agencies, will continue, with much of the actual work being in state level institutions. The intent of the deliberative body would be to ensure that those processes, public and private, essential to realizing the benefits of science are achieved. The group would define goals and objectives germane to U.S. society, but the analogy is with the deliberate decision by Japan to take "extraordinary measures for the promotion of the electronics industry."

It is suggested that this group also set forth a proposed organizational arrangement whereby implementation of its recommendations may be carried out. Such an arrangement could be an explicit organizational entity, perhaps one designed along the lines of the National Technology Foundation (NTF) proposed by Congressman George Brown. NTF, as set forth in H.R. 3749, 1981, would consolidate most federal technology-related functions, and it would give deliberate direction to non-military technological change. The assumption, however, is that the center of action with respect to technological innovation should be at the federal level, and the operating procedures are set forth accordingly. It is my contention that states should exercise such leadership, with the federal government supporting and otherwise enhancing public and private action within states through research, tax, environmental and other policies and programs. If the state view prevails, the operating style of NTF, as proposed, will need modification.

An alternative to NTF as such could be redefinition of the roles of the Science Advisor to the President, of OSTP, and of NSF, and also of links with other federal agencies, plus deliberate inclusion of representation from state and local governments.

No attempt is made here to define in detail the structure and functions required in the future. The point being made is that a serious void pertaining to technological innovation exists at federal, state and local levels of government, and that changes are required to fill it. Reliance on the existing structure of science and technology will not be sufficient if the United States is to reassert its leadership.

Conclusion

I will conclude my remarks with a re-emphasis on the need for a comprehensive leadership role for the federal government. I have purposely avoided mentioning specific budget figures today, although I share your concerns about the impact of cuts in science programs. I have stayed away from details such as recommending specific priorities for the federal government, although I have strong biases in support of many vital projects. Rather, I wish to emphasize the need to move away from the state of complacency into which we, as a country, have apparently settled. I want to emphasize the need to urgently recognize the implications of the competitive scientific and technological environment of the world today. Ultimately, we must act as a nation calling upon the totality of its resources--in local, state and federal government, and outside of government as well--to reestablish ourselves as a strong leader in transforming the results of basic research into goods, services and other functions of direct benefit to society.

I contend, again, Mr. Chairman, that the federal government must set in motion the processes for redesigning our structure of science policy and organization now as it did beginning nearly 40 years ago. Only the challenges of this decade do not include landing on the moon, nor do they include the creation of a superior war machine. Rather, these challenges involve merging the technical and organizational capabilities of our nation--the talents and skills of our best minds--so that our R&D efforts may be united toward essential social, economic, and cultural purposes.[1]

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[1] For additional background information concerning the role of states in making more effective case of the results of basic scientific reserach, see Federal - State Science Policy: Present and Future, background paper prepared for the Task Force on Technological Innovation, National Governor's Association, 444 North Capitol Street, Washington, D.C., 20001, February, 1982.

Mr. **Brown**. Thank you very much, Dr. Lindsey. Your statement is a very valuable contribution to the subcommittee.

Dr. Casteen?

STATEMENT OF DR. JOHN CASTEEN, SECRETARY OF EDUCATION, COMMONWEALTH OF VIRGINIA, RICHMOND, VIRGINIA

Dr. **Casteen**. Mr. Chairman, in beginning I should perhaps correct the record by indicating that I am the Secretary of Education in Virginia and that my obligations in the State actually touch all of education from day care centers through postdoctoral programs and not simply the sciences

The prepared statement that I have delivered to the committee has to do largely with how the system of education and especially the high schools support manpower capable of producing the kinds of expertise and knowledge in science and research that other witnesses have discussed this morning. Congressman Brown asked earlier today whether the American and science manpower pool is, in fact as large as it should be and compared our manpower pool to that in Japan.

I would like to begin by making some basic observations about educational strategy in support of the sciences and then move through some suggestions that developed originally under Governor Hunt's leadership in the National Governors Association.

The Japanese educational strategy since World War II has been described as an investment in human capital, an investment that proceeded from the assumption that one could best revitalize an industrial society by building the human expertise necessary to attract investment dollars rather than assuming that the demand for the labor itself must exist before one can justify the educational investment. While the analogy is not altogether perfect, most of the discussion this morning of the relationship between the educational system and our capacity in the sciences has had to do with the demand for the outcome of education, with the level of education we can justify, not with the assumption that the general level of education and advanced education in the sciences together predict our ability to consume the resources that people develop in an educational system.

The various member Governors of the National Governors' Association have been concerned about the relationship between school programs and scientific, technological and general industrial development. They have pursued the assumption that problems in our schools can be identified and solved, and that solutions will require certain kinds of State and Federal collaboration on one end as well as certin kinds of State and local collaborations on the other end.

In looking at the progress of students through courses that support the sciences and technology, certain key choices that students make in our schools matter a great deal. The choices matter in part because of the structure of our curriculum. We fragment the disciplines and teach biology in 1 year, chemistry in another, algebra 1 year, geometry in another, whereas most industrial nations build curricula in spiralized sequences.

In other countries mathematics is developed continuously in controlled sequences so that students pursue mathematics in conjunction with the sciences. In our school system two key mathematics choices seem to drive students' later choices. One is the choice whether the student takes algebra I in either grade 8 or grade 9. That is a choice to which most students say no at each of those points of choice and it is a choice that in the aggregate no more than a third of our students make in the affirmative, which is to say that most students reject the mathematics course that undergirds following courses in science and math. A negative choice in grade 8 or grade 9, predicts that most students will not, in fact, complete a rigorous course in mathematics or sciences later. The choice of calculus in grade 12 or the freshman level of college has the same impact. The yes choice is made by only about 8 percent of our students, which is to say in the other direction that 92 percent of our students who face that choice elect not to prepare for subsequent options in science, engineering or mathematics.

We have analogous problems with regard to the languages of scientific discourse. With substantial federal encouragement during the 1960's, we have developed fairly good competence in elementary scientific German for most students in relevant of disciplines. We have developed negligible expertise in Russian and in Japanese and in other languages in which scientific discourse takes place and the evidence is that even in light of strategic concerns about those languages we are making little headway in developing a scientific community that is able to carry on discourse in languages that have been increasingly important.

The two-thirds who vote no on mathematics and the sciences in the eighth and ninth grade and the 92 percent voting no at grade 12 and freshman college level are effectively saying that they do not wish to pursue certain kinds of disciplines in colleges. Poor enrollments kill courses. That was true of Russian, which we tried to develop in the schools in the latter part of the 1960's.

I have tried to address the obligation of the States in this regard and that of the Federal Government in the prepared document. The State obligations fall into three broad areas.

One is accreditation. States control most accreditation regulations either directly or through the regional associations. Our accreditation standards more often address physical realities, how many, how big, in what places, than academic standards. We rarely dictate what schools should do as part of the accreditation process. Instead we address how schools should be in the physical or material sense.

State government must set requirements. Those requirements ought to grow out of a calculated vision of the future that State governments wish to foster, especially the kind of future that was described this morning as being in the national interest in the sciences and technology.

As a footnote, because of what I see as faulty standards, enrollments are poor both with regard to college preparatory programs and with regard to vocational programs. Putting those programs together we account for fewer than half the students who graduate from the schools each year. The remainder appear in the general track and the kindest description is that we do not know what those students are taking.

States pay most of the bills for the kind of education that supports the scientific development that your committee is exploring. That obligation to pay is a matter of enlightened self-interest for the States. Our experience is that local control over schools works almost exactly to the extent that local resources are committed to supporting schools. State government has an obligation to reinforce the ties that link schools and the community and these ties include certain kinds of accountability. Our relatively modest record in enrolling students in recent years in mathematics and science, a record that has deteriorated by 50 percent since 1970, indicates to a large extent the weakness of the links that States have forged between schools and the communtiy.

The Federal role is a role that has to be determined, as I think the other witnesses suggested, by a combination of the national interest on one side and the location of resources on the other side.

Certain kinds of investment are implicitly appropriate to the Federal Government. Certain kinds of resources happen to belong to the Federal Government and not to other sectors of the government or society.

Our Governors have concentrated this year on student financial aid, whose gradual decline and, perhaps more importantly, unreliability because of the delayed issuance of regulations by the Department of Education in Washington and because of the special impact on our least affluent people of uncertainty as to what the level of support will be, have become major problems.

Governors are concerned also about basic research and about certain kinds of applied research. Here I have one suggestion to offer for the committee's consideration. The chief justifications for Federal involvement in these areas are high cost, cost that may exceed the ability to pay of the private sector or the State government, and the speculative or specialized nature of the return that such investments will produce.

The argument made this morning by Dr. Martin on behalf of investment in Department of Defense scientific research, I think, exemplifies that kind of reasoning. The suggestion I would like to make is that while also addressing, as you must, the issue of Federal support for basic scientific research and for certain kinds of applied research, the committee also look to the possibility or returning to the former Federal tradition, especially in the 1960's, of applying research dollars to the question of school effectiveness. The fact is that how schools do what they do impacts directly on our ability to meet needs in the scientific areas.

I would like to suggest that Federal programs have included both advantageous or good programs and poor programs and that one characteristic of the poor programs is that the research behind them has rarely had to answer the right questions. The question, I think, ought to be what do we accomplish in our schools, how do we measure that acomplishment and having measured it how do we apply the human capital that we have developed by that investment.

Mr. Chairman, thank you.

[The prepared statement of Dr. Casteen follows:]

STATEMENT OF JOHN T. CASTEEN, III

Secretary of Education, Commonwealth of Virginia

Subcommittee on Investigations and Oversight
Subcommittee on Science, Research, and Technology

Committee on Science and Technology
U.S. House of Representatives

September 30, 1982

I have come today in behalf of the National Governors' Association and the Commonwealth of Virginia to share with you some recommendations concerning the partnership of the states and the federal government in support of science and technology. Most of these recommendations were developed initially in a symposium held on August 12, 1982, at Afton, Oklahoma, as part of the annual meeting of the National Governors' Association. Governor Hunt of North Carolina served as chairman of that symposium, and arranged my own participation and that of Chairman Lewis Branscomb of the National Science Board and former Presidential Science Advisor Edward David. My remarks today have to do primarily with how we can foster both excellence and productivity in mathematics and science education in secondary schools.

With regard to the interests and roles of the states and their governors in improving mathematics and science education, I want to make three assertions: first, that the policy issue begins and may be said to end in the questions who takes what courses and who teaches them; second, that our educational values changed during the past fifteen years to the detriment of science and technology; third, that solutions to the problem of low enrollments in mathematics, the sciences, and other academic disciplines in our high schools will come, and that these solutions will originate in political forums.

Other speakers at the N.G.A. meeting described a decline over the course of fifteen years in the proportion of our high school students who complete serious programs of courses in mathematics and science prior to graduation, and a concurrent rise during the same period in the proportion of our college students who require remedial or high school courses in order to learn what they did not learn in high school. The materials prepared for that meeting by the staff of the Education Commission for the States include reports on the extent to which enrollment has slipped in the basic disciplines. These reports concentrate on mathematics and science, but one should note that the decline in enrollment in foreign languages is steeper and in many ways more troubling. In the subjects seen as "hard", many students have chosen in an era of all-but-free choice in our schools to avoid tough courses and pursue easy courses.

Ample evidence suggests that the decline in enrollment in the tough courses has been steepest in the most recent years. Stanford Dean of Admissions Fred Hargadon's now-famous letter to the nation's secondary school counselors and principals, in which Hargadon cautioned that declining enrollment in the basic courses in high school predicts a virtual catastrophe in higher education, came only in 1978. The best known responses to the decline, such as the College Board's Educational EQuality Project, the Southern Regional Education Board's quality initiative for schools in the southeast, and the Council for Basic Education's intensified program in support of school reform are all developments of the last eighteen months or two years.

One needs also to observe that the eight percent or so of our high school students who take calculus, which is to say, in effect, those who actually complete four full years of our most serious high school mathematics courses, appear to be better prepared than ever. I was formerly Dean of Admissions in the University of Virginia. In my work there, I found that these top students could succeed in virtually any collegiate program. The irony was that so few students actually completed the most rigorous courses offered in our high schools. Our best students compare favorably in international competitions in mathematics and other disciplines with those from other countries. Indeed, one speaker at the N.G.A. meeting reported that U.S. high school math teams have regularly won the International Math Olympiad, which might be called the world series of school mathematics competition. Ironically, however, our Math Olympiad teams frequently draw their members from only five high schools, all of them well known as anomalies in our educational system.

A reasonable analysis of curriculum development in our high schools during the last decade or so might go something like this: Students have taken fewer courses. Students have taken fewer hard courses. Students have suffered. Their test scores, their records of success in college, their records of success in the basic academic disciplines, and their difficulty in finding productive jobs after high school all underscore truisms that we in education should have discovered years ago: people don't test well on what they don't know, and most people don't know what they haven't studied.

Statistical analysis available from the Education Commission of the States will indicate that no more than a third of our high school students take a full, rigorous program of courses to prepare for post-secondary study, while almost two-thirds go on after high school. When one adds that only about one-third, or slightly more, of those who go on to college ever graduate, and that students who require remedial (high school) courses in college graduate at a rate far below the average, the cost is painfully clear: we need to do the job right the first time around, when students are in high school. To delay is to diminish our students' prospects of achieving their goals in life, and of course also to diminish the national capacity to compete in a world market where industrial expansion comes more readily in the brain-based industries, the new technologies, than elsewhere.

Other information prepared for the N.G.A. meeting suggests the extent to which teachers govern solutions to the problems of enrollments in mathematics, the sciences, and other subjects. Like it or not, we have chosen in recent years not to pay adequate salaries to our teachers, not to provide well conceived advanced training for veteran teachers, and not to address squarely the conditions in which teachers work. For many teachers, recent years have not been happy times. Our schools have not worked as well as they can. Too often, they have been poorly disciplined, poorly planned, poorly administered, and poorly supported. Ultimately, we get what we pay for. If we do not like the product, we ought to look at least as deliberately to the price we pay as to the reforms we want to undertake.

When I say that the changes in schooling over the course of the last fifteen years derive largely from changed perspectives, I mean that we have lived through a period of uncertainty as to the purposes of schooling. The last fifteen years have seen constant turmoil over how and what we teach. The fads of professional educationists have perhaps too often replaced academic substance and rigor in the classroom. Recent research in the field of education tells much of the story. We have paid little attention to subject content since the beginning of the decade of the 'seventies, and essentially no attention to how advocates of the most common innovations (mini-courses in place of substantive courses of a more traditional design, social promotion, trendy alternatives to substantive courses) have documented their successes and failures. All too often, the research supporting academic programs in high schools has resembled popular psychology more than academic discourse.

Consensus has been rare in recent years. The attack on testing has been until very recent months a fact of life in our schools. Despite carefully researched and well-reasoned responses to testing's critics, including the analysis published in recent months by the National Academy of Science, many Americans continue to deny that schooling ought to face a bottom line, that we ought to prove the validity of what we do. We have preferred to punish the bringer of bad news, the test or tests that say that schooling used to do and can do a better job for most of our students, rather than to confront the news itself.

In this era of turmoil about methods, we have also seen disunity develop in our university faculties. Our scientists and mathematicians disagree among themselves with remarkable vehemence as to the bases of their own disciplines. On many campuses, the pure mathematicians carry on all-but-open war with the computer mathematicians. Collegiality often becomes merely a myth. Indeed, during the last decade many universities developed two mathematics departments, one for the pure or theoretical mathematicians, and another for the computer mathematicians, who are seen as having accepted peace without honor in the war with the engineers. In the sciences, the chemists and the physicists who ought logically to be allies of the engineers and the computer scientists are often arranged in opposition to these technologists. And on it goes. Too often, we set academic policy to protect turf as much as to foster the growth of ideas, and few universities are well organized in 1982 to deliver the kind of academic substance that our society as a whole needs.

Finally, we have come in recent years to separate out mathematics and sciences from the liberal arts. We have pretended that the liberal arts are the humanities and the social sciences, and that in some way they do not embrace the entire range of bookish learning that the liberal arts include. The founders of American public education--Jefferson, Franklin, men and women in localities throughout the country--saw mathematics and science as elements of the bookish background for citizenship, not as separable topics to be mastered only by the gifted and talented. As in the universities, the dispute in the community at large has been about turf, about who owns mathematics and science, not about more substantive matters. Confused by our confusion, more than a few students have excused themselves from major components of education.

In this context, one note of clarification is essential. Today's fad in education is "back-to-the-basics." I am not talking today about basics because we cannot afford mere basics. The reports of the National Assessment of Educational Progress persuade me, as they do most of my colleagues who work with me toward school reform, that our schools have dealt successfully with the basics through about grade seven or grade eight, when students begin to choose courses for themselves, often without adequate guidance, rather than to follow fixed curricula.

Instead of seeking basics, we need to see to it that our secondary schools support complexity, that we encourage students to take courses that can propel them beyond surface knowledge or understanding. In the end, schooling that will protect America's future must guarantee that citizens are highly competent, knowledgeable, and critical. Basics do not support achievements of this order. Good state government and support for optimal educational achievement for all citizens must go hand-in-hand.

I have predicted that solutions to these problems will originate in political forums for three reasons. First, schooling has political purposes that we rarely acknowledge nowadays. The founders of American public education sought to prepare citizens for sound participation in the democracy, to see that our system of government would thrive and survive. That purpose has endured. Second, education has been essentially still in the water for about a decade. The causes are several: scant leadership from higher education since the onset of its Vietnam-era anomie; economic deprivation, combined with the effects of diminishing enrollments; the realization throughout industry and government that education has in many respects failed to deliver the products (properly educated citizens, adaptable research products, and the like) for which we pay when we fund education. Political leaders and members of governing boards, often political men and women themselves, have in this climate both opportunity and good motives to lead education toward change.

The potential for state action to remedy many of these problems clearly exists. Virtually all states set standards for accreditation of elementary and secondary schools. Yet state rules more often count "things" (books on the shelf, windows or square inches in windows,mop washers) than detail what schools must do. We rarely specify what level of mastery our teachers are to achieve in their subject areas or what level of success in the classroom. Indeed, little about our system of accreditation has to do with the classroom except in a physical sense: we make sure it is there. Instead, we resort to simple quantitative measures of the least important indicators of educational quality. State leader can and should press now for simpler, more pragmatic rules. The rules need teeth. They must guarantee that we pay for excellence when we fund our public schools, state or local, elementary, secondary, or postsecondary, and that we reward excellence, not inertia.

State leaders and especially governors have a vested interest in reforming the curriculum. The status quo more often distracts students from productive academic choices than attracts students to them. Curricula are rich in abbreviations of traditional academic subjects, in ill-conceived alternatives to Advanced Placement or other rigorous courses, and in empty choices that track students away from courses that lead to opportunities at the next level of education.

The importance of the basic choices cannot be overemphasized. In most states, most students choose in the eighth grade not to take algebra I. Because of the sequence of courses necessary to qualify students to take calculus in the senior year of high school, the choice in the eighth grade not to take algebra I amounts to a choice not to take calculus in high school. This choice implies that the student will not enter our most selective and most prestigious colleges and universities, and especially not our most rigorous schools of engineering and applied science. Students who do not take algebra I in the eighth grade face the same choice in the ninth grade, and once again most choose not to take the course. This second choice effectively excludes students from most technological fields of study after high school, because virtually all require a sequence of mathematics and science courses built on algebra. Students can go back and make up missed courses, but few do.

Efforts at curriculum reform are underway, sometimes by state action, as in California and other states, sometimes by regional action, as in the South through the Southern Education Regional Board, sometimes nationally, through programs such as the College Board's Educational EQuality Project, the school reform programs at the Council for Basic Education, and the National Science Board's commission. Each will in its way succeed. Each deserves careful consideration from governors and state legislators who want their schools to graduate citizens who will command the skills that our emerging economy, an economy that drives profits on human intellect more than on the work we do with our hands and backs, requires.

For many of the states, these issues amount to basic dilemmas involving the links between our schools and our communities. Several states have already created task forces to address education generally or education as it relates to science and technology. You know about Governor Hunt's accomplishments in this area in North Carolina. My own Governor Robb has undertaken an analogous initiative in Virginia, with special emphasis on our need to reeducate labor forces in parts of the state where we are losing traditional industries and where we must attract investment dollars from the new technologies if our economy is to thrive. Arizona's joint program of educational and industrial development is often cited as a model to be imitated elsewhere. There are other examples. These initiatives matter because they encourage state leaders to set directions in collaboration with business, labor, and education, and because they acquaint the public with the urgent need to support schools sufficiently, to demand proper performance of school personnel, and to develop meaningful academic standards.

Governors are, of course, uniquely able to forge links between schools and industry and business. Competition for new business investments is daily business in our state capitols. All of us acknowledge the importance of attracting new kinds of employers, new kinds of capital, new kinds of tax resources. In this competition, some states win and some states lose. In recent years, those most likely to win have been those whose governors have taken the most aggressive stance in demanding a realistic return on the educational investment. Schools and industry can share expertise. Chemists, engineers, and other technological professionals can teach with minimal special training. Teachers can conduct certain kinds of research for local industries, or in other ways contribute their expertise. North Carolina proved these points two decades ago with the Research Triangle. Arizona is proving them now in a different way. Schools and industry can share space. Governor Robb has urged our schools and colleges to work with industry in creating industrial/educational campuses in which the campus will be the work place and the work place the campus. Massachusetts is developing funding for an ambitious program of this kind.

What is the federal interest? As a state official, I might prefer to suggest that federal dollars ought to pay for everything, but I know, as you must, that advice of that kind serves neither the interest of the nation nor the interests of the states. Most kinds of educational policy ought to be made by the states, or indeed by units of government within the states--by local school boards, by teachers within local schools. Distributed governance, combined with the assumption of financial responsibility at the lowest level of government that can finance specific educational activities, has served us well in most parts of the country.

Yet certain policy issues must belong to the nation. Our current inability to compete effectively in certain industries in the international market suggests that the national interest demands federal involvement. And that consideration persuades many thoughtful Americans that Congress must look at our schools' and colleges' ability to support industrial redevelopment by producing highly competent men and women as well as research expertise of the appropriate kinds.

In the same sense, certain financial commitments are either beyond the reach of the states or within the immediate purview of the Congress. Student financial aid seems to me to be a good example of the former kind of commitment, and support for basic research, of the latter.

The federal government's willingness since the adoption of the G.I. Bill to assert that financial need alone cannot stand between students and the opportunity to learn is a high point in our educational history. For sundry reasons, many perhaps sound, recent appropriations show that the withdrawal from student financial aid promised by the Administration is upon us. While the loss is not yet as dramatic as some educators have claimed, it is causes problems whose solutions may lie beyond the reach of the states. The damage done by the federal government's gradual withdrawal from certain kinds of financial aid and (what may prove even more disastrous) by the inability of the U.S. Department of Education to issue its regulations governing financial aid in a timely fashion is to my mind apparent this fall. Early reports from the public colleges of my own state indicate that enrollment is strong overall but soft in the colleges that enroll most of our poorer students. At least one of our traditionally black universities reports especially serious problems for students whose families cannot afford to pay the cost. That this condition results not from Congressional action but from the bureaucracy's inability to do business on time may well be an educational issue on which the Congress should act.

My concern about this loss is not merely social, although it embraces social issues. With many of you, educational leaders in the states believe that financial barriers to educational opportunity diminish the effectiveness of our system of government and the quality of our national life. But an equally urgent concern is that many states cannot afford to pick up the cost of financial aid programs that suffer or die here in Washington, and other states need time to develop new programs. The actual cost of meeting the full need probably exceeds the capacity of the states to pay, just as it exceeds the capacity of many students to pay.

And these conditions have implications for educational policy. The cost of picking up former federal aid programs diminishes the states' ability to deal with the academic needs that I detailed above. The progressive cost of delaying academic reforms in our schools while we address the short-term emergency in financial aid inevitably accrues as much to the nation as to the states. To the extent that school reform must wait until the states build resources to pay costs that have historically belonged to the federal government, federal action must be said to diminish our chances of developing necessary expertise and technologies.

Support for basic research also seems to me to be a federal obligation. The cost of building the academic base necessary if we are to revitalize American technology is beyond the resources of the states, and the investment is so speculative that industry, plagued by recession and the high cost of money, defers commitments that in happier times it might make. Perhaps even more to the point, the benefits of such research in the basic sciences belong to the nation, not to any one state or region.

Finally, let me appeal to you to let our notion of basic research include research into what works in schools. In this area, the Congress may have special reasons to assume at least some of the cost. In suggesting that recent educational research has more often supported fads than substance, I mean to include the research that lay behind many of the federal adventures in our schools in recent years. I do not mean to say that the federal accomplishment is trivial. It is not. The National Science Foundation, Head Start, and the activities supported by the National Endowment for the Arts and the National Endowment for the Humanities, to name only a few federal undertakings, deserve credit for much that is good in our schools.

But not all federal experiments in education can claim the empirical bases of these program or show results like theirs. Not all federal impact on schools has been constructive. Opinions differ on the value to students of such federal initiatives as mainstreaming or what is sometimes called (grandeloquently, perhaps) bilingual education. My own is that this value is yet to be shown. Too often, the zeal of the bureaucracy in proposing legislation and in promulgating regulations and distributing money has not been sustained by demonstrations that the particular method or program promoted by the federal agencies has ever worked anywhere. We need to get beyond letting happy talk replace logical and factual analysis of what works.

In school and college mathematics and the sciences we need the kind of leadership that federal agencies (like the National Science Foundation) provided so effectively in the years following Sputnik when we made our first effort to revitalize education and thus sustain technology. That earlier effort worked as well as it did in part because it supported solid research into school effectiveness, into course content. More recent efforts that have failed and that may actually have damaged schooling did so in large part because their proponents justified them by answering the wrong questions. If federally supported research can show the nation again what works in schools and help set national directions for schooling for the remaining years of the century, the Congress will have laid the necessary foundations for the scientific and technological renaissance that your inquiry seeks to foster.

Mr. **Brown**. Thank you very much, Dr. Casteen. I think both of you gentleman has given us a very important component of this overall policy of setting strategies or priorities in science that we haven't really tended to give sufficient attention to, and we very much appreciate that.

Dr. Lindsey, your State has been pointed to frequently as a leader in this matter of focusing on science and technology and I seem to recall that you have selected certain key areas within the overall field to emphasize such as biotechnology, for example. Could you give us a little description of what the process is within the State by which you try to focus on certain particular areas in science to maximize your effect there?

Dr. **Lindsey**. Well, central to our organization is what we call our North Carolina Board of Science and Technology. This is a 15-member board that Governor Hunt chairs. By virtue of being his Science Adviser, I am the executive director of that board. The remaining members are mostly scientists from research institutions plus representation from local governments and components of private industry.

This group has laid out a rather extensive program that we are trying to pursue. Within that is a component concerned with exploration on the frontiers of science. We have examined the potential of scientific exploration and concluded that two fields have tremendous signficance in the years ahead, one of which is microelectronics and the other is molecular biology. Research in education has been pursued in North Carolina in microelectronics for some time, but almost 4 years ago, as we began to assess this field, we decided that we had to put more effort into it and emerge as among the leading states, or to move along in a second- or third-rate position.

We decided that, given the close proximity of some of our institutions to each other in the Research Triangle area, we could develop a means by which those institutions could share the extremely expensive scientific equipment that is required in microelectronics research and education and lower the cost per instituion, or per person trained, by virtue of such savings we can compete favorably with other major institutions in the country.

So, we created the Microelectronics Center, located it in the research triangle park, and arranged for six institutions to participate in the use of equipment for research and education purposes. We are developing a microwave system interconnecting these institutions and the center so that they will have two-way communications by television or other means.

We put the chancellor of the participating academic institutions on the board of that institution and make it a corporate not-for-profit entity and added a few other citizens of the State to the membership. The president of the Triangle Institute is chairman of the board.

We have yet to finish a building and get everthing in place, but that is how we did it. The state legislature saw fit to put $24.4 million into the center.

We followed a somewhat simple process in the field of biotechnology, although we are not as far along. We simply could not afford to fund both the microelectronics and the biotechnology centers at the same level at the same time. But we hope to provide much stronger support to the biotechnology center soon.

But it is in pulling together the research and education programs of the participating institutions which is important.

Mr. BROWN. Thank you. I would like to recognize Mr. Gore for any questions he might have.

Mr. GORE. Thank you, Mr. Chairman.

I will be brief.

Let me extend my apologies to the witnesses. The schedule this week has been unexpectedly hurried because of the new deadline of tomorrow to close the pre-election session.

Dr. Lindsey, I would like to welcome you back to the Investigations and Oversight Subcommittee. We appreciate your conclusions in the past and admire the work that you do in North Carolina, and I hope you will take our best wishes to Gov. Jim Hunt, who I think is just an outstanding public official, and the fact that your State is so far in front in this important area is real credit to the leadership that you have had and that you personally in part are providing.

I appreciate your appearance as well, Dr. Casteen, and I have talked with Governor Robb and he hasn't been in office as long as Jim Hunt, but when he got in office, he was off the mark right away and recognized the importance of this effort. I wish you the best and I am impressed with how much headway you have made.

Do you tend to look with favor on technologies that your State university research labs are actually strong on or is it the reverse, where you will encourage State research entities to reflect their efforts to enhance their capabilities to include coming technology like biotechology?

I will ask you both that question.

Dr. LINDSEY. I guess I would put it this way. As we assessed the fields of microelectronics and biotechnology, we concluded that we were not keeping pace with change, that other institutions in the country were moving on ahead. If we wanted to continue to have outstanding institutions in this field, we had to do something more. And rather than simply sink a lot of money in each institution in increased equipment, additional faculty or whatever it might require, we sought ways to, as I described with microelectronics, pool the effort wherever we could.

I might add one other interesting point in the realm of science that led to our creation of these two centers. Both of those fields require extensive interdisciplinary work. They are not fields that an individual specialized scientist in one discipline can go off and do his or her thing for years and come up with all the answers or even part of them. The centers help foster strong interdisciplinary research, not just within one institution, but among several.

Mr. GORE. Thank you.

Dr. Casteen?

Dr. CASTEEN. I needed a moment to organize my thoughts becuase we are, in fact, in the middle of that issue in Virginia. I think I can break it down into several different issues that illustrate the problems States face.

Our institutions are heavily committed to types of research that were developed at great expense, and adaptation of existing facilities or development of new facilities places a severe financial strain on any one institution.

We do not have as centralized a system of governance in education as North Carolina, nor do we have a history of institutional collaboration.

As a consequence, we receive questions from various parts of the State for research support and find ourselves having to broker that support in ways that puts some strain on the State Treasury. In the Tidewater area of Virginia, there is a demand for laser technologies that must be driven off labortory work in universities. Our centers for that kind of work are at Charlottesville and Blacksburg, several hours away from the Tidewater area.

Faculties do not wish to relocate their facilities. There is second the problem that in the technical areas industrial demand is often for relatively low-grade scientific activity and facilities concerned with research in the pure sense often dislike having to divert energy from that activity in order to commit it to another activity.

To take a very simple indication of the kind of thing that a State confronts, emerging new technologies demand a great deal of in-service training for graduate engineers who need to be constantly immersed in ongoing research.

It becomes difficult to persuade faculties that they ought also to provide what is from their perspective sometimes a relatively low-grade and perhaps even scientifically destructive kind of instruction on-site in a factory because industry needs that kind of contact with the academic world in order to continue industry's own development.

A great deal of recent technological advance has occurred outside the academy. The development around Boston, for example, in the labs of the high technology companies often took place without formal links to higher education. From time to time, both sides assert a certain amount of proprietary interest that prevents us from developing the collaboration that the marketplace clearly requires if the marketplace is to flourish.

Mr. Gore. Well, thank you. That is very interesting.

Is there a danger that State universities will shift R&D emphasis toward applied rather than basic research that responded to State science priorities? I know you have wrestled with that. I know your university people have, and I am interested in your views.

Dr. Casteen. Let me make an observation about the type of scientific research that State universities cultivate. One can overdo the distinction, but we support basis and applied research in somewhat different ways. We have a tradition of applied research. The best place to see it is in the land grant universities. Much of our research activity has, in fact, been directed toward the commercial marketplace.

Other universities, especially private ones, are more committed to theoretical or basic research. They require some kind of special incentive to take part at all in the applied end of research. We find ourselves engaging in a kind of tug of war as we try to develop new attractions for industrial development because we cannot, in fact, commit the resources of all of our universities at the same level in all parts of the State to employers who might request collaboration within the academy.

Dr. LINDSEY. I don't foresee that to be a danger. First, I think in some respects the dichotomy between basic and applied research is overdrawn.

Second, if you create a situation in which you have outstanding scientists, engineers, other types of people involved in this entire process, a highly stimulating environment results and vigorous minds will pursue these lines of interest, whatever they may be, that grow out of that stimulation, much of which would normally be classified as basic research.

Third, we find in the affiliates' arrangements with industry, for example, that faculties and inevitably pushed toward more fundamental lines of investigation. Several industrial firms, when supporting such research collectively, desire it be sufficiently removed from direct application. Each firm can then feel that the insights that may grow out of such basic research, can be taken back to its own laboratory for applied research and development purposes.

Finally, I believe that the distinct role of the Federal Government is to continue to provide strong support for the more fundamental lines of research. Defining those lines and how much to expend on each is, of course, an extremely difficult process.

Mr. GORE. Well, thank you very much. I appreciate your testimony here today.

Thank you very much, Mr. Chairman.

Mr. BROWN. All right. I have no further questions, gentlemen, and I think you have had a long morning here, so we will conclude the hearing at this point and if we have any further questions requiring your help, we will sumbit them to you in writing.

Thank you very much.,

[Whereupon, at 1 p.m., the subcommittee was adjourned.]

SETTING PRIORITIES FOR SCIENCE

WEDNESDAY, DECEMBER 8, 1982

HOUSE OF REPRESENTATIVES, COMMITTEE ON SCIENCE AND TECHNOLOGY, SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY, AND THE SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT

Washington, D.C.

The subcommittees met, pursuant to call, at 10:45 a.m., in room 2322, Rayburn House Office Building, Hon. Albert Gore, Jr. (chairman of the Subcommittee on Investigations and Oversight) presiding.

Mr. GORE. The subcommittees will come to order.

I would like to apologize to our witnesses and guests for the delay. I am told you were forewarned that there was other business this morning in the Democratic Caucus. Let me just explain that there was an important and significant change in the manner in which constitutional amendments are dealt with by the Congress. When an issue of that character arises, it is extremely significant.

Congressman Walgren is still taking part in that consideration, and we may have a vote coming up very shortly, in which event I will seek to keep this proceeding going through the shuttle routine.

Since World War II the United States has maintained leadership in many fields of science and technology, including biomedical technology and computer technology. We take great pride in the accomplishments which increase our well-being and improve our lives.

The Federal Government has played a key role in the advance of knowledge through long-term real growth in funding for basic and applied research in all scientific areas.

In the past decade, after adjusting for inflation, basic research funding increased by 2 percent, applied research by 3 percent, and development research by 3 percent. Throughout the 1960's, the U.S. ratio of civilian R&D to gross national product steadily increased, and after a temporary decline in the early 1970's, surpassed its 1960's levels, reaching an estimated 1.66 in 1981.

Today, economic conditions have caused many to question the role of the U.S. Government in the promotion of science and technology. The Reagan administration has stated that federal R&D level, except defense, are contingent on general economic conditions and fiscal constraints. Indeed, last year Federal funding for basic research dropped 2 percent, accounting for inflation.

Last year, the Director of the Office of Science and Technology Policy, Dr. Keyworth, told the full committee that in light of the current economic situation, real growth in science cannot continue and that cutbacks in science are not likely to harm the health of American science.

Furthermore, OSTP has informed the Congress that R&D will be subject to more rigorous criteria of excellence and pertinence. In that connection, we regret that Dr. Keyworth could not be present at this hearing to elaborate on his views. We hope to hear from him on another occasion soon.

Other nations have incorporated science advancement as a major component of their industrial policies. Industrialized countries such as Japan and France are subsidizing research and development for new products and services desired in expanding world markets. Such increased international support for science requires even greater U.S. scientific resourcefulness and strategies for maintaining U.S. competitiveness.

Within this country as well, increasing competitiveness among the States to attract high technology industries has resulted in new State science and technology programs for upgrading scientific manpower and encouraging commercial usefulness of university research. Thus, the States will play an increasing role in the direction of science policy.

While we must strive for increased innovation and corporate productivity, the Federal Goverment must continue to play a vital role in developing noncommercial advances in science which serve the greater public interest by protecting our health, environment, and our national defense.

Debate is fast emerging regarding the role of public interest as a component in the direction of scientific and technological research. The National Science Foundation's recent "Five-Year Outlook" emphasizes the stress which science is currently suffering due to the lack of public support for science. We must establish linkages between science and the public in dialogues regarding the role of science in the United States.

We will focus on a number of issues related to these concerns in today's hearing:

No. 1, how can the public interest be reflected in Federal R&D decisionmaking, in the choices and in the development and application of technology?

No. 2, how do we best determine the optimal funding level for basic research?

No. 3, what are the proper respective roles of government and private industry in funding applied research and development?

No. 4, should the United States target certain R&D for international economic competition?

No. 5, what lessons can the Government learn from our leading corporate research efforts, and what public policies should result?

We would like to thank our distinguished witnesses who are appearing here today.

I would like to recognize my colleague, the gentleman from Ohio, Mr. Shamansky.

Mr. SHAMANSKY. Thank you, Mr. Chairman.

I appreciate the opportunity to welcome Dr. Ronald Paul, president of the Battelle Memorial Institute, and George Johnson, a vice president of Battelle. I think your remarks were especially appropriate because Battelle has raised at least to me and then through me a bill called the Automobile Research Competition Act, the whole question of how you free up or inspire the private sector to invest its funds and then reward it if in fact it meets this criteria.

We have an analogy with the way we develop military aircraft. We even have an analogy in history in Britain in the 1900's when they would have competition for the advances that enhanced the industrial revolution in England.

So, it seems to me that instead of having the Federal Government do everything directly, we might well use the idea which originated, as far as I am concerned, at Battelle, of a competition, establish their criteria and then encourage the private sector to meet that. In case they make the criteria, they would then get reimbursed.

We certainly have got to find a better method of inspiring the private sector to risk its capital, but at least with the chance of getting repaid.

I welcome the distinguished Dr. Paul as a witness.

Mr. GORE. The gentleman from New Mexico, Mr. Skeen.

Mr. SKEEN. Thank you, very much, Mr. Chairman.

I, too, regret the delay this morning, but it is understandable. It is what makes this thing work or not work, depending on how you look at it.

I want to commend you for the hearings. I think this particular topic is extremely important. I see in the Wall Street Journal of yesterday that we are not buying frogs anymore for dissection, biological labs anymore, because of our inability to keep up with the pace of the money that we have expended on science in the United States.

I think that is extremely regrettable. I think that is what made this country as great as it is today.

I want to welcome the two gentlemen who are here today, Dr. Paul and Dr. Patel. We appreciate you being here, particularly from that sector of the science community representing the private sector.

We appreciate you being here. I want to welcome you and am looking forward to your testimony.

Thank you, Mr. Chairman.

Mr. GORE. Thank you.

Without objection, I would like to include the statement of Congressman Doug Walgren, the chairman of the Subcommittee on Science, Research and Technology, in the record at this point.

[The opening statement of Mr. Walgren follows:]

OPENING REMARKS OF HON. DOUG WALGREN, CHAIRMAN, SUBCOMMITTEE ON SCIENCE, RESEARCH AND TECHNOLOGY

I join Chairman Gore in welcoming our distinguished witnesses to this second day of hearings on "Setting Priorities for Science".

The opening day, held September 30, gave us the benefit of hearing from two groups of witnesses with important experience in this matter: officials of Federal agencies and academic experts.

Today we will hear three distinguished representatives of American industry. It has long been recognized that many of the dramatic advances made by industry in this century are science based, and that a significant part of the science on which those advances has been based has been done by industry itself.

Because the most important reason for the Federal Government's support of scientific research is our expectation that it will contribute to technological advance, we can no doubt learn much about priority setting from the outstanding practitioners.

For congressional committees, who each year must review the budgets of the Federal science agencies, priority setting questions do not involve the choice of individual research projects or the support of individual scientists. Rather, the priorities that must be established involve choices between funding levels for disciplines and subdisciplines, between individual large and costly "big science" facilities, and ultimately the difficult question of "how much is enough" to meet the several national objectives which together constitute the rational for federal support of science.

I look forward to hearing the views of today's witnesses and to our discussion with them of the applicability of those views to our priority setting process in the Congress.

Mr. GORE. Let me also note for the record the regret of the two subcommittees that our third witness this morning, the chairman of the board of the National Association of Manufacturers, Mr. Bernard J. O'Keefe, for reasons not quite clear to the subcommittees decided at the last moment last night, late, that he didn't want to testify and pulled out at the last minute. We don't exactly know why. It is somewhat puzzling. We had hoped that the NAM would have some contributions to make to this proceeding.

[The prepared statement of Mr. Bernard O'Keefe was submitted at this time for inclusion in the record:]

TESTIMONY OF
BERNARD J. O'KEEFE
CHAIRMAN
NATIONAL ASSOCIATION OF MANUFACTURERS
"SETTING PRIORITIES FOR SCIENCE"

Introduction

I am Bernard J. O'Keefe, Chairman and Chief Executive Officer, EG&G, Inc. and Chairman of the Board of the National Association of Manufacturers.

We commend the Committee for holding these hearings and thank the Chairman and his staff for affording NAM the opportunity to express its concern over R & D priorities.

The NAM is a voluntary membership organization representing approximately 12,000 companies which employ a majority of the country's industrial labor force and which produces over 85 per cent of the nation's manufactured goods. The Association is also affiliated with an additional 158,000 businesses through the National Industrial Council and NAM's Association's Department.

Research and Development expenditures by industry will increase in 1983 despite the conditions of a poor economy. By Battelle Memorial Institute's estimate, U.S. industry will spend $41.4 billion on R & D in 1983, about 7.6 per cent more than this year. One of the reasons for the increase is a change in the attitudes of corporate executives to R & D goals over the last two years.

Through most of the decade of the 70s, a major portion of R & D expenditure was characterized as defensive. With the passage of stringent environmental laws and very wide interpretation of the concepts of product liability on the part of the courts, most companies had to pay a great deal of attention to increased environmental constraints on the performance of their products in the marketplace and the possibility of being held liable for the performance of their products in situations which went far beyond the product specifications originally contemplated. This was particularly true in automobiles, chemicals and pharmaceuticals. Since cooperative research was inhibited by anti-trust laws, each company had to solve its problems in isolation, resulting in a very inefficient use of manpower and money.

In the past two years, attitudes have been changing. Much of the research needed to comply with environmental laws has been carried out, and industry has modified its specifications to reduce the risk of liability. Further, the Administration and the Congress are making a sincere effort to reduce needless government regulation, and the courts are beginning to realize that overly-liberal interpretations of liablity laws are not in the best long-range interest of the consumer who must pay the price of overly-conservative product design.

During the present recession American industry has come to realize the extent to which we are in a world economy, that we must fight to stay ahead, and that many of our basic industries must be restructured. The trick of economic adjustment in times like these is to nourish new industries while sustaining the old ones. The new industries are in electronics, bio-technology, communications and other high-tech areas. We are stepping up R & D in those industries to maintain our competitive international edge. These sciences are developing markets of their own in new and exciting fields such as home computers, word-processing and bio-technology which is delivering improved medicines such as interferon. But they are also important to restructuring and improvement of productivity in our basic, capital intensive industries. In 1983, fifty-two per cent of the $100 billion industrial capital spending will be for computers, communication systems and electrical instruments. This market is just beginning.

Payback periods vary widely with the company and the product line. In minicomputers and semiconductors, product life cycles can be as short as three years, so early payback is essential. In pharmaceuticals and chemicals, five to ten-year paybacks are adequate. With today's high interest rates and our recent history of inflation, it is difficult to see how any unregulated industry can afford paybacks longer than ten years. On a discounted cash flow basis the value of a dollar invested today becomes vanishingly small after ten years.

One of the most important points for Congress to consider is the wasteful, expensive, non-competitive aspects of anti-trust law on R & D. New ventures, new products are risky, the riskiest dollars a company can spend. They are risky not only because the project may not turn out to be feasible, but also because the effort can be superseded by domestic or international competitors. Our international competitors, particularly Japan, are exempting long-range cooperative R & D from their monopoly laws. They invested $100 million in cooperative R & D for random access memories and now have the bulk of the market. They are currently investing $300 million in research on fifth generation computers. After the R & D is finished the information is widely disseminated. There is plenty of room for competition in engineering, manufacturing, finance and marketing.

We may have as many as a dozen companies working on the same problems, but the work is wasteful of talent, time, and money because each must duplicate the work of the others. Nor does it have the critical mass of brainpower available when scientists are allowed to work in collaboration. It will be devastating to our economy and to our national security and prestige if we have to go to Japan for the latest computers in 1990, because the individual U.S. companies could not afford to do the R & D in this country. Relief from these structures will be paramount to our scientific success in the years to come.

There has been a trend among U.S. companies engaged in research and development to move away from the long-range, basic research. This "blue-sky" research is always uncertain of yielding marketable results, and can be very expensive over the long run. The NAM believes that because government is doing much of this work, there is even less chance that the results of such research can benefit the economy. We believe that changes in the patent policies of government to allow exclusive licensing of government owned patents--which have been estimated to number some 28,000--could help bring the results of government-funded research closer to the marketplace.

To summarize, we believe that among the major priorities for research and development in the private sector in the U.S., encouragement of cooperative research through changes in anti-trust, and the opening up of government owned patents through exclusive licensing would contribute substantially to maintaining U.S. preeminence in science and technology.

Mr. GORE. At any rate, let me call our first witness. Our first witness is Dr. Ronald Paul, president of Battelle Memorial Institute, Columbus, Ohio.

Dr. Paul, we are delighted to have you here. Our colleague, Mr. Shamansky, has sung the praises of Battelle for lo these 2 years, and we have had the benefit of a lot of the work that you all have done. We appreciate your assistance on this occasion.

Without objection, your entire prepared statement will be included in the record in full. We invite you to present it. If there are any portions you care to summarize, feel free to do so.

Welcome.

[The biographical sketch of Dr. Paul follows:]

DR. RONALD S. PAUL

A Biographical Sketch

Dr. Ronald S. Paul is President and Chief Operating Officer of Battelle Memorial Institute -- an organization of 7,300 scientists, engineers, and supporting specialists engaged in worldwide research, educational, and technology development activities. A pioneer in contract research, Battelle is the world's largest nonprofit independent research institute.

In his present position, Dr. Paul has responsibility for the Institute's five major operating components in the United States and Europe and related corporate functions.

Since joining Battelle in 1965, Dr. Paul has held a succession of key management positions: Director of the Battelle Seattle Research Center (1968), Director of the Pacific Northwest Division (1971), Corporate Vice President - Operations (1973), Senior Vice President (1976), Executive Vice President and Chief Operating Officer (1978), and his present position (1981).

Before joining Battelle, Dr. Paul performed and managed research in nuclear physics, reactor physics and instrumentation, nondestructive testing, radiological physics, and atmospheric sciences for 13 years at the General Electric Company's Hanford Atomic Products Operation in Richland.

Dr. Paul is a member of the American Physical Society and the American Nuclear Society and of the scholastic honoraries Pi Mu Epsilon, Sigma Pi Sigma, and Sigma Xi. He has authored a number of technical papers and was a Lecturer in modern physics at the Richland Graduate Center for several years. In 1962, he served as a technical consultant to Japan for the International Atomic Energy Agency.

Active in civic affairs, Dr. Paul is a member of the Board of Trustees of the Center of Science and Industry in Columbus, the Board of Directors of the Columbus Cancer Clinic, the Board of Trustees of Children's Hospital Research Foundation in Columbus, the Executive Board of the Central Ohio Council of the Boy Scouts of America, and the Planning, Review, and Evaluation Organization in the Coalition for Cost Effective Health Services. In addition, he is a member of the Board of Trustees of Denison University, Granville, Ohio, and the National Advisory Board of The American University in Washington, D.C.

Dr. Paul earned his B.S., M.S., and Ph.D. degrees -- all in physics -- at the University of Oregon.

STATEMENT OF DR. RONALD PAUL, PRESIDENT, BATTELLE MEMORIAL INSTITUTE, COLUMBUS, OHIO

Dr. PAUL. Thank you.

Mr. Chairman and members of the subcommittee, I am really pleased to be able to appear here today and comment on the setting of priorities in science. I am doing this out of personal experience and the experience of Battelle.

Battelle Memorial Institute is a private, independent, nonprofit but tax-paying corporation. I think we are quite unique in that latter regard. Our basic purpose is the advancement and utilization of science for the benefit of mankind through technological development and education.

We operate four large, multi-purpose laboratories located in Columbus, Ohio, Richland, Wash., Geneva, Switzerland, and Frankfurt, West Germany, and a number of specialized facilities elsewhere.

Our division in the State of Washington is also a prime contractor to the government for management and operation of the Pacific Northwest Laboratory of the Department of Energy.

The several Battelle laboratories perform research, development, and related technical activities on a diverse array of subjects through contracts with Government agencies, commercial organizations and trade associations. These are worldwide.

We also have a project management division headquartered in Columbus, which established and manages the Office of Nuclear Waste Isolation for the Department of Energy. This office is the key element in the national waste terminal storage program, which is concerned with the ultimate geologic disposition of the residue from operation of commercial nuclear power plants.

Our corporate headquarters and two subsidiaries are located in Columbus. The latter are devoted to bringing technology into commercialization through the development, patenting, and licensing of inventions, and through establishing and assisting new technological business enterprises.

Battelle's annual volume of work funded by others will exceed $425 million this year. This work is primarily of a mission-oriented and problem-solving nature. However, embedded within it there also is basic scientific research and a dependence upon basic scientific research done by others. Therefore, national science priorities impact what we do, and what we and similar organizations do influences and should influence national science priorities.

I will bring to your attention some of the features of the process of bringing science into utilization which can be influenced by congressional action or inaction.

Assume for a few moments a generalized process which begins with the discovery of scientific knowledge and ends with quality of life. We all know in reality that the process is a highly complex, dynamic interactive effort which has many alternative routes, branches, and dead ends. The beginning of this model centers upon the discovery and advancement of scientific knowledge. Three of the crucial areas of Federal policy at this pole that merit your concern are these:

First, Federal support for education and education-related research in science and engineering needs to be equitably distributed nationwide, primarily, but not exclusively, through academic institutions.

One reason is that no citizen with aptitude should be denied the entry opportunity for becoming a productive scientist or engineer through the circumstances of birth or location.

Another reason is that the level of scientific knowledge of the general population needs to be increased because of the rapidly increasing technological dimensions of our society. The constructive utilization of science seems to be blocked much more frequently by ignorance than by knowledge.

Second, those relatively few scientists and technologists who have unusual competence, creativity and even genius must be undergirded by the Federal Government. They make the discoveries and lead the advancements which open entirely new fields of opportunity. They are the spearheads for our global leadership in science. They must be identified and nurtured.

Third, the Federal Government must continue to make long-term commitments to a few areas of science that require very expensive equipment and facilities—for example, in studying the universes of very small dimensions of atomic, nuclear, and subnuclear matter; or in exploring and studying the universe of very large dimensions of the space sciences.

Only advanced industrialized nations as ours can afford such research. Thus, we have a responsibility to choose wisely these long-term projects, to stay with them consistently and long enough to assure them sufficient opportunity to be productive, and to courageously shut them down when they have outlived their scientific usefulness.

For convenience, I will simply term the middle area in our discussion model "applied research and development." The major participants include industrial laboratories, private institutions such as Battelle, government in-house and contractor-operated laboratories, and applied engineering schools. Many Federal policies affect them. I will highlight only three of the several impacts I consider of high importance.

First, the government scientists and engineers in agencies which contract for R&D services need to be competent, to be professionally motivated, and to have continuity in their assignment comparable to the duration of the contracts they help manage.

The importance of the sponsor function is something we at Battelle know well from more than 50 years' experience as a leading pioneer and proponent of contract research. No matter how able the performer of the R&D is, the entire project or program suffers when the sponsor or client is technically weak, has ill-defined or shifting goals, or frequently changes personnel.

I and many of my contemporary colleagues are deeply concerned about the increasing prevalence of these situations during the past 10 years.

The examples of fine technical sponsorship that do exist in parts of the government need to be broadly emulated. Great results can be achieved when there is a partnership between government and contractor based on mutual professional respect, confidence, integrity and trust. Great waste of resources can occur when any of these characteristics is lacking.

Decisions on R&D budget appropriations need to factor in the ability of the cognizant agency to professionally manage the funds. Where deficiencies occur, corrective actions should be taken and supported.

Long-term measures should be devised and implemented to assure that we have a cadre of scientists and engineers within the Government who are highly professional and dedicated, but perhaps fewer in number than now.

Second, the policies governing competitive procurement of R&D by Government agencies need periodic evaluation and improvement. In our experience, competitive procurement works well in our system when the end goals, or the deliverables, as we say, are clearly describable, and when the sponsoring agency has technical competence.

Problems can arise if the contract is more for research than for development; that is, more to seek discovery and understanding rather than to solve a particular problem. Or problems arise if the technical competence of the sponsor is weak. Then the risk exists of having nontechnical factors, such as price and ancillary goals, unduly influence the procurement decision to the detriment of scientific and technological process.

Another concern is the possibility and, in fact, the likelihood that in highly competitive procurements the number of potential bidders is not reduced rapidly enough. Consequently, the economic and human resources consumed in the bidding process by the losers may easily exceed the resources applied to the project by the winner of the contract award.

To minimize this nonproductive consumption of scarce resources, we need streamlined procedures for competitive procurement of R&D which are more cost effective for both the Government and the potential bidders.

The agency people tell us that their burdensome procurement procedures are required by Congress. Those procedures need to be balanced by appropriate levels of opportunity for sole source negotiation of R&D contracts.

Third, it is important that Government R&D procurement policies recognize the necessity and importance of internal research and development, commonly referred to as IR&D. All applied R&D laboratories—industrial, nonprofit, academic, or Government—need some financial capacity to do work at their own discretion on subjects that are not yet directly supported by funding from their mission-oriented sponsors. This IR&D is an important seedbed for germinating new ideas which can lead to new technologies or new applications for existing technologies.

Often IR&D also is a principal means for applied research organizations to have a few basic research scientists on their staff who become an important linkage between the originators and the users of fundamental scientific discoveries and knowledge.

Although the level of internal research and development within Battelle is less than what we would like, being only several percent of our total activities, it has a strong influence in shaping our science priorities. Government policies should liberalize rather than restrict the allowability of IR&D as an indirect expense of Government R&D contracts.

The output of the flow of scientific knowledge includes commercial technological processes and products, and a variety of methods and devices used by operational Government units.

The interaction of national science policy with these end uses in both the private and Government sectors is very complex and beyond the scope of this brief statement. Therefore, I will limit my remarks to several observations made through the window of Battelle's contract research business upon this scene.

During the seventies, much of our contract research was defensive in nature; that is, it derived from regulatory legislation in the environmental, health, and safety fields, for example. Most such projects had social merit when considered individually.

However, the totality of their magnitude and pace diverted public and private resources that might otherwise have been applied to the maintenance and improvement of our industrial productivity and our international competitive position.

During the last several years—and I am pleased to report this—our mix of contract research has become more balanced. An increasing share is now aimed at new product and process development and at productivity improvements. These trends and supportive Government policies can become major contributors to economic recovery.

Finally, patent policies as they apply to Government R&D need to provide incentives for the private sector to make those investments needed for commercialization. For example, concern over granting exclusive rights to one licensee is sometimes a barrier. We deal with that in Battelle in our own commercialization efforts by requiring the licensee to diligently pursue commercialization in order to obtain and retain exclusivity.

Because in many of our endeavors we act as a bridge between Government and industry, I thought you might be interested in how we set our own R&D investment priorities. Briefly, we perform an analysis based on five perspectives:

One, an examination of current and potential future technologies for growth or significant impact. I say as an aside that this includes examining the sciences concerned for those sensitive areas where breakthroughs or new knowledge will create considerable leverage in opening up new fields of technology.

Two, a review of the needs and interests of our clients in the marketplace.

Three, an assessment of gaps or voids in the R&D efforts of Government and industry.

Four, an estimate of the timeframe for the application of these technologies and the duration of financial support.

Five, an evaluation of our own skills and interests to build on our strengths or build up our staffs.

This is not a rigid and formalized evaluation, but it does involve a structured assessment conducted by teams involving scientists and technologists in our United States and European divisions, coupled with our marketing representatives.

This corporate process is an addition to the planning and priority setting by our divisions. Their bottoms-up efforts are supplemented by our corporate top-down assessments. Where we believe that additional attention needs to be given to a research area or subject, we support the effort with corporate funds at one or more of the divisions.

In the final selection process, financial payback does not dominate the decisions. It is important because we must be financially viable. But of greater relevance to us is the benefit or significance of the potential results.

In conclusion, my remarks have been made from the vantage point of a large, independent applied research organization. We have a stake in the welfare of basic science on one hand and in the welfare of the users of science and technology on the other hand. I have noted several ways in which national science priorities impact the total process of bringing scientific knowledge into utilization.

I will be pleased to answer any questions you may have.

[The prepared statement of Dr. Paul follows:]

STATEMENT OF

DR. RONALD S. PAUL
PRESIDENT
BATTELLE MEMORIAL INSTITUTE

before the

SUBCOMMITTEE ON INVESTIGATIONS AND OVERSIGHT

and the

SUBCOMMITTEE ON
SCIENCE, RESEARCH, AND TECHNOLOGY

of the

COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

December 8, 1982

Mr. Chairman, and members of the subcommittees, I appreciate this opportunity to discuss the setting of priorities in science.

Battelle Memorial Institute is a private, independent, nonprofit, but tax-paying corporation with over 7000 staff members worldwide. Our basic purpose is the advancement and utilization of science for the benefit of mankind through technological development and education.

We operate four large, multipurpose laboratories located in Columbus, Ohio; Richland, Washington; Geneva, Switzerland; and Frankfurt, West Germany; and several specialized facilities elsewhere. Our division in the State of Washington also is a prime contractor to the government for management and operation of the Department of Energy's Pacific Northwest Laboratory. The various Battelle laboratories perform research, development, and related technical activities on a diverse array of subjects through contracts with government agencies, industrial firms, and trade associations.

We also have a Project Management Division headquartered in Columbus which established and manages the Office of Nuclear Waste Isolation for DOE. The Office of Nuclear Waste Isolation is a key element in the National Waste Terminal Storage Program for the ultimate disposition of the radioactive material resulting from the operation of commercial nuclear power plants.

Our corporate headquarters and two subsidiaries are located in Columbus. The latter are devoted to bringing technology into commercialization through the development, patenting, and licensing of inventions; and through establishing and assisting new technological business enterprises.

Battelle's annual volume of work funded by others will exceed $425 million this year. This work is primarily of a mission-oriented and problem-solving nature. However, embedded within it there also is basic scientific research and a dependence upon basic scientific research done by others. Therefore, national science priorities impact what we do, and what we and similiar organizations do influences and should influence national science priorities.

I. The Process of Science Utilization

I wish to bring to your attention some of the features of the process of bringing science into utilization which can be influenced by congressional action or inaction. Assume for a few moments a generalized process which begins with the discovery of scientific knowledge and ends with understanding, products, or processes which improve or preserve the quality of life. We all know in reality that the process is a highly complex, dynamic interactive effort which has many alternative routes, branches, and dead ends.

A. The Discovery Phase

The beginning of this model centers upon the discovery and advancement of scientific knowledge. Three of the crucial areas of federal policy at this pole that merit your concern are these:

- First, federal support for education and education-related research in science and engineering needs to be equitably distributed nationwide, primarily, but not exclusively, through academic institutions. One reason is that no citizen with aptitude should be denied the entry opportunity for becoming a productive scientist or engineer through the circumstances of birth or location. Another reason is that the level of scientific knowledge of the general population needs to be increased because of the rapidly increasing technological dimensions of our society. The constructive utilization of science seems to be blocked much more frequently by ignorance than by knowledge.

- Second, those relatively few scientists and technologists who have unusual competence, creativity, and even genius must be undergirded by the federal government. They make the discoveries and lead the advancements which open entirely new fields of opportunity. They are the spearheads for our global leadership in science. They must be identified and nurtured.

- Third, the federal government must continue to make long-term commitments to a few areas of science that require very expensive equipment and facilities -- for example, in studying the universes of very small dimensions of atomic, nuclear, and subnuclear matter; or in exploring and studying the universe of very large dimensions of the space sciences. Only advanced industrialized nations such as ours can afford such research. Thus, we have a responsibility to choose wisely these long-term projects, to stay with them consistently and long enough to assure them sufficient opportunity to be productive, and to courageously shut them down when they have outlived their scientific usefulness.

B. The Development Phase

For convenience, I will simply term the middle area in our discussion model "applied research and development." The major participants include industrial laboratories, private institutions such as Battelle, government in-house and contractor-operated laboratories, and applied engineering schools. Many federal policies affect them. I will highlight only three of the several impacts I consider of high importance.

- First, the government scientists and engineers in agencies which contract for R&D services need to be competent, to be professionally motivated, and to have continuity in their assignment comparable to the duration of the contracts they help manage. The importance of the sponsor function is something we, at Battelle, know well from more than 50 years of experience as a leading pioneer and proponent of contract research. No matter how able the performer of the R&D is, the entire project or program suffers when the sponsor or client is technically weak, has ill-defined or shifting goals, or frequently changes personnel. I and many of my contemporary colleagues are deeply concerned about the increasing prevalence of these situations during the past ten years.

  The examples of fine technical sponsorship that do exist in parts of the government need to be broadly emulated. Great results can be achieved when there is a partnership between government and contractor based on mutual professional respect, confidence, integrity, and trust. Great waste of resources can occur when any of these characteristics is lacking.

  Decisions on R&D budget appropriations need to factor in the ability of the cognizant agency to professionally manage the funds. Where deficiencies occur, corrective actions should be taken and supported. Long-term measures should be devised and implemented to assure that we have a cadre of scientists and engineers within the government who are highly professional and dedicated, but perhaps fewer in number than now.

- Second, the policies governing competitive procurement of R&D by government agencies need periodic evaluation and improvement. Competitive procurement works well in our system when the end goals, or the deliverables as we say, are clearly describable; and when the sponsoring agency has technical competence. Problems can arise if the contract is more for research than for development; that is, more to seek discovery and understanding rather than to solve a particular problem. Or problems arise if the technical competence of the sponsor is weak. Then the risk exists of having nontechnical factors such as price and ancillary goals unduly influence the procurement decision to the detriment of scientific and technological progress.

Another concern is the possibility, and, in fact, the likelihood, that in highly competitive procurements the number of potential bidders is not reduced rapidly enough. Consequently, the economic and human resources consumed in the bidding process by the losers may easily exceed the resources applied to the project by the winner of the contract award. To minimize this nonproductive consumption of scarce resources, we need streamlined procedures for competitive procurement of R&D which are more cost-effective for both the government and the potential bidders. The agency people tell us that their burdensom procurement procedures are required by Congress. Those procedures need to be balanced by appropriate levels of opportunity for sole source negotiation of R&D contracts.

- Third, it is important that government R&D procurement policies recognize the necessity and importance of internal research and development (IR&D). All applied R&D laboratories -- industrial, nonprofit, academic, or government -- need some financial capacity to do work at their own discretion on subjects that are not yet directly supported by funding from their mission-oriented sponsors. This IR&D is an important seedbed for germinating new ideas which can lead to new technologies or new applications for existing technologies. Often IR&D also is a principal means for applied research organizations to have a few basic research scientists on their staff who become an important linkage between the originators and the users of fundamental scientific discoveries and knowledge. Although the level of IR&D within Battelle is less than what we would like - being only several percent of our total activities - it has a strong influence in shaping our science priorities. Government policies should liberalize rather than restrict the allowability of IR&D as an indirect expense of government R&D Contracts.

C. The Application Phase

The output end of the flow of scientific knowledge includes commercial technological processes and products, and methods and devices used by operational government units. The interaction of national science policy with these end uses in both the private and government sectors is very complex and beyond the scope of this brief statement. Therefore, I will limit my remarks to several observations made through the window of Battelle's contract research business upon this scene.

- During the seventies, much of our contract research was defensive in nature; that is, it derived from regulatory legislation in the environmental, health, and safety fields, for example. Most such projects had social merit when considered individually. However, the totality of their magnitude and pace diverted public and private resources that might otherwise have been applied to the maintenance and improvement of our industrial productivity and international competitive position. During the last several years, our mix of contract research has become more balanced. An increasing share is now aimed at new product and process development and at productivity improvements. These trends and supportive government policies can become major contributors to economic recovery.

- Finally, patent policies as they apply to government R&D need to provide incentives for the private sector to make those investments needed for commercialization. For example, concern over granting exclusive rights to one licensee is sometimes a barrier. We deal with that in Battelle by requiring the licensee to diligently pursue commercialization in order to obtain and retain exclusivity.

II. R&D Investment Priorities

Because in many of our endeavors we act as a bridge between government and industry, I thought you might be interested in how we set our own R&D investment priorities. Briefly, we perform an analysis based on five perspectives:

- An examination of current and potential future technologies for growth or significant impact.

- A review of the needs and interests of our clients in the marketplace.

- An assessment of gaps or voids in the R&D efforts of government and industry.

- An estimate of the time frame for the application of these technologies and the duration of financial support.

- An evaluation of our own skills and interests to build on our strengths or build up our staffs.

This is not a rigid and formalized evaluation, but it does involve a structured assessment conducted by teams involving scientists and technologists in our United States and European divisions coupled with our marketing representatives.

This corporate process is an addition to the planning and priority setting by our divisions. Their bottoms-up efforts are supplemented by our corporate top-down assessments. Where we believe that additional attention needs to be given to a research area or subject we support the effort with corporate funds at one or more of the divisions.

In the final selection process, financial payback does not dominate the decisions. It is important because we must be financially viable. But of greater relevance to us is the benefit or significance of the potential results.

In conclusion, my remarks have been made from the vantage point of a large independent applied research organization. We have a stake in the welfare of basic science on one hand, and in the welfare of the users of science and technology on the other hand. I have noted several ways in which national science priorities impact the total process of bringing scientific knowledge into utilization.

I will be pleased to answer any questions you may have.

Mr. **Shamansky**. Thank you, Dr. Paul.

I believe before questions we will have Dr. Patel testify.

Thank you very much.

Mr. **Shamansky**. Dr. Patel?

[The biographical sketch of Dr. Patel follows:]

Dr. C. Kumar N. Patel

C. Kumar N. Patel is Executive Director, Research, Physics Division at Bell Laboratories in Murray Hill, New Jersey.

In 1964 Dr. Patel invented the carbon dioxide laser, today the most widely used laser industry, medicine, and scientific research. In 1969, Dr. Patel pioneered in the development of the so-called "spin-flip Raman laser," and he later used this type of tunable laser to detect air pollutants at extremely small concentrations. His work has led to many other laser systems, including some which are now being explored for possible use in nuclear fusion power generation.

Dr. Patel has received numerous honors and awards. These include the Optical Society of America's Adolph Lomb Medal, the Franklin Institute's Stuart Ballantine Medal, the American Chemical Society's Coblentz Prize, the Association of Indians in America's Honor Award, the Institute of Electrical and Electronic Engineers' Lamme Medal, the National Academy of Engineering's Zworykin Award, Texas Instruments Foundation's Founder's Prize, and the Optical Society of America's Townes Medal.

Dr. Patel is a member of the National Academy of Sciences and the National Academy of Engineering. He is a fellow of the Institute of Electrical and Electronics Engineers, the American Physical Society, the Optical Society of America, the American Academy of Arts and Sciences, and the Association for Advancement of Arts and Sciences. He is a foreign fellow of the Indian National Science Academy, and he is an honorary member of the Gynecologic Laser Surgery Society. He is a member of the Board of Trustees of the Aerospace Corporation.

In 1961 Dr. Patel joined Bell Laboratories, where he began his research on gas lasers. This work broadened to include nonlinear optics, molecular spectroscopy, and pollution detection. He became head of the Infrared Physics and Electronics Department in 1967, Director of the Electronics Research Laboratory in 1970, and Director of the Physical Research Laboratory in 1976. In 1981 he assumed his present position, in which he continues to pursue a vigorous program of personal research.

Dr. Patel received the B.E. degree in Telecommunications from the College of Engineering in Poona, India. He received the M.S. and Ph.D. degrees in Electrical Engineering from Stanford University in 1959 and 1961, respectively.

STATEMENT OF DR. C. KUMAR N. PATEL, EXECUTIVE DIRECTOR OF RESEARCH-PHYSICS, BELL TELEPHONE LABORATORIES, MURRAY HILL, N.J.

Dr. **Patel**. Thank you very much, Congressman Shamansky, members of the committees.

I am pleased to have this opportunity to comment on the process of setting priorities for scientific research at Bell Laboratories and the extent to which that process may be applicable at the Federal level.

I should remind you that Bell Laboratories is an R&D organization, part of AT&T, and the part that I am going to comment about, namely the scientific research, is a small fraction of the total R&D effort. It is approximately 8 percent.

At a time when our Nation is seeking to exercise fiscal restraint to facilitate economic recovery, and at a time when our Nation needs to apply its worldwide leadership in science and technology to assure future economic growth, the committees' attention to setting priorities for science is especially important and very welcome.

I hope that my comments on Bell Laboratories' approach to the subject of priorities in science is helpful to you. In my written statement I have included examples of Bell Labs' unparalleled record of accomplishments in basic scientific research which has resulted from our approach to scientific priority setting.

I will not detail these to you right now, unless you have specific questions on them. I will, however, point out that over the years we have developed an effective process for setting research priorities, which has played a major role in our success.

The principal features, which facilitate this process include three very important factors, which I would like to call the three F's.

The first of the F's is funding, sustained funding, to be more precise. The second F is focus, or the organization as a whole. The third F is freedom for the individual scientist.

Sustained funding, which is the first F, is important because scientific research is a long-term and inherently risky endeavor. We at Bell Laboratories are sustained by our owners, AT&T and Western Electric, and by our partners, the Bell operating companies and the Long Lines.

The Bell System has long realized that steady funding protected from upswings and downturns of the economy is essential to the success of long-term research programs. That sustained funding is important can be easily concluded from considering the time that elapses between the inception of the research effort and the discoveries and inventions that flow out from them and subsequent implementation at a laboratory level.

The overall focus of our work is provided by our mission, given to us by our owners, that mission is to support the business of AT&T and Western Electric, the business being the information business.

Our owners choose certain technologies based on such criteria as the economic potential, marketability, demand for services, manufacturing considerations and so on, but—and this is the very important part—they delegate to Bell Laboratories responsibility for determining research priorities.

Within Bell Labs, management allocates scientific resources in response to opportunities identified by our scientists. Management also considers the degree of scientific challenge, the expected time between inception and payoff, relevance to technologies of interest to the company, and cost effectiveness.

The principle is that the individual scientist and the managers make those decisions that they are best qualified to make. Thus, at Bell Labs there is an all-important separation between the sources of funding and those who provide focus for the research, a rather unique situation almost anywhere in the whole world.

This separation works because of three important factors: One, outstanding managers who understand research and who understand the course of science, seen from the past, present and toward the future; an effective mechanism for evaluating research, an ongoing process; and the immersion of our research effort in a viable research and development structure of Bell Laboratories which is driven by real world needs.

In research, I would like to remind you that research results, as well as the process of arriving at them, are both important. For that reason, an effective mechanism for evaluating research in an ongoing fashion is extremely important in setting priorities.

Freedom for individual scientists, the third F, is important because in the final analysis it is the individuals who make discoveries and inventions. This freedom at Bell Laboratories is provided within an arena of awareness of our overall mission, which I mentioned just a few moments ago.

The individual scientists set and evaluate specific priorities for the research. This is so because we at Bell Laboratories strongly believe that the future cannot be predicted from the present.

If specific priorities were left to managers with development responsibilities for exploiting technology, it is likely that research, although contributing incrementally to existing technologies, would not lead to discovery of radically new principles, new processes or new devices.

It is that aspect—namely, accumulation of new knowledge, discovery of radically new principles and processes—that Bell Laboratories' research effort is all about.

The same freedom for scientists, that allows them to evaluate and set priorities, simultaneously provides an important focus through the free flow of information and people between our research and development areas, and through the contact of our researchers with the operating parts of our companies.

These are not only formal linkages. These are day-to-day linkages between people working in different parts of our enterprise: research, development, manufacturing and service.

In my prepared statement I have offered a couple of examples of how focus and freedom have contributed to breakthrough discoveries and inventions. These examples are, first, the UNIX computer operating system, and the second one, the carbon dioxide laser. These demonstrate that the combination of funding, freedom and focus is a synergistic combination and that each one of them benefits from the others.

Finally, the all-important question: Can what has worked at Bell Labs be projected at the Federal level? Members of this committee should recognize that a research effort succeeds best when excellence is a primary criterion for priority setting.

It is because, as I mentioned earlier, the new knowledge, new discoveries, new processes are created are made possible by individuals themselves. Priorities are best set by scientists who are held accountable for their decisions. A very important part of science is in fact this accountability.

The legislative process of consultation, compromise and consensus may work for development of technology, as the Japanese have demonstrated, but when the goal is new scientific knowledge, which is what I am addressing right now, we must rely upon the expertise of the scientists themselves.

So, the Federal policies then should be limited to setting objectives within broad fields, such as computer science, such as genetic engineering, solid-state physics, and so on.

But these reservations aside, I believe that the principles underlying Bell Labs' priority setting process are applicable at the Federal level. To summarize, these principles are:

First, that the best scientific research, being a long-term endeavor, requires sustained funding;

Second, that the overall goals of the research effort must be understood and accepted by both researchers, managers and those who provide the funding;

Third, that the individuals involved in priority setting make those decisions that they are best qualified to make;

Fourth, that no matter how large an organization or how coordinated and well-managed the research effort, new knowledge is created by individual scientists; and

Finally, most important of all, discoveries and inventions cannot be legislated.

Thank you, gentlemen. I will be happy to answer your questions.

[The prepared statement of Dr. Patel follows:]

STATEMENT OF

C. KUMAR N. PATEL

My name is Kumar Patel. I am Executive Director of Research, Physics Division at Bell Telephone Laboratories Inc., Murray Hill, New Jersey.

I am pleased to have this opportunity to comment on the process of setting priorities for scientific research at Bell Laboratories, and the extent to which that process may be applicable at the Federal level.

Bell Laboratories' unparalleled record of accomplishment in basic scientific research is, I believe, already well known to the Committee. It includes the invention of the transistor and numerous contributions to solid-state electronics, the conception of the laser and many related discoveries, fundamental work on polymer plastics including the stabilization of polyethylene, the invention of permanent magnet alloys, the first electrical digital computer and fundamental contributions to digital transmission and switching, pioneering work in information theory, statistical quality control, microwave and radar technology, communications satellite technology, and many other achievements. Seven Bell Laboratories scientists have received Nobel Prizes.

An effective process for setting research priorities has played a major role in our success. This process assures that our scientific research effort serves our broad mission: to provide the research and development the Bell System needs to offer high-quality telecommunications and information services at reasonable and competitive cost. The priority-setting process itself results from the complex interplay among three important factors; which I would like to call the "three F's": sustained funding, focus and freedom.

Scientific research is a long-term, inherently risky endeavor that requires long-term consistent, non-fluctuating financial support. We at Bell Laboratories are sustained by our owners, AT&T and Western Electric, and by our partners, the Bell Operating Companies. Their investment in research, development and engineering is substantial; this year it will total $2.02 billion, of which $156 million is allocated for research. The record shows that this investment consistently has yielded a handsome return. In fact, the Bell System has long realized that steady funding, protected from "upswings" and "downturns" in the economy, is essential to the success of long-term research programs where risk is great, but potential payoff may be far greater.

The second "F," the overall focus for our work, is provided by the mission given to Bell Laboratories by our owners, AT&T and Western Electric. They direct Bell Labs technology effort to support what they perceive to be their business: the broadly defined "information business." From a wide variety of available technologies they choose to implement certain technologies by applying such criteria as economic potential, marketability, demand for services, manufacturing considerations, long-term potential, and so on. However, the responsibility for determining research priorities is delegated to Bell Laboratories. This process of priority-setting at the highest level defines, in broad terms, the areas of science and engineering likely to be of commercial value, and guides our research effort toward broad overall goals.

Within Bell Laboratories, research management allocates scientific resources in response to opportunities identified by scientists requesting resources and the overall priorities of the organization. Management provides what it considers an appropriate proportion of the available space, equipment, personnel, and other resources. Management also considers the proposed research, the expected time between its inception and its eventual payoff, relevance to the specific technologies of interest to the company, and cost-effectiveness. These broad priorities are reviewed by those managers in the research area with overall responsibility for the various aspects of the research effort. Finally, the scientists performing the research decide how best to use the resources.

The principle behind this scheme of shared priority-setting is that individual scientists and managers at various levels of the institution make those decisions they are best qualified to make. The process recognizes that science and technology are closely intertwined; that research drives technology toward new products and services; and that in research the process of arriving at results is often just as valuable as the results themselves. The process of arriving at results is especially important when results are unanticipated or the studies produce negative results. Even when studies are unsuccessful, the documentation of the process allows us to evaluate what is not possible and why. Unexpected results also have tremendous impact on broad scientific and technological fronts far beyond the initial goals.

One of the unique features responsible for the success of basic research at Bell Laboratories is the separation between the sources of funding and those who provide focus for research. This separation exists almost nowhere else in the world, whether funding is provided by government or industry, except in a few foundations which grant funds to individual scientists who then determine the focus of their own work. There are three reasons the separation works at Bell Laboratories: outstanding research managers, an effective mechanism for evaluating research, and the immersion of our research effort in a wider research and development structure driven by real-world needs.

Those who manage the research effort at Bell Laboratories not only have been outstanding scientists themselves, but often continue their research even in their management positions. Thus they are simultaneously both managers and "hands-on" scientists, and as such they are well equipped to make decisions regarding both long- and short-term priority setting. Further, rather than the usual method of evaluating research projects upon completion, managers at Bell Laboratories evaluate projects continuously from their inception. This allows them to "fine-tune," or alter the direction of a project, on the basis of interim findings. Finally, the immersion of our research enterprise within a larger research and development structure keeps managers keenly aware of Bell Laboratories overall technical mission. Managers recognize that within that overall mission the research enterprise plays an important role in maintaining contact with outside researchers--both in academia and in industry--and in keeping in touch with the Bell System's technology needs. Thus our research effort is focused to create new knowledge, to keep Bell Laboratories in close contact with advanced science, to explore applications of new science to telecommunications and information technology, and to build links with our customers, our colleagues, and the scientific community.

The core of our research effort is a group of about 800 scientists whose broad goal is the creation of fundamental knowledge to support the future technology needs of the information age, including the discovery of new principles, the invention of processes and materials, and the exploration of connections that may not be obvious between the new principles, processes and materials. Managers at Bell Laboratories realize that it is these individuals who discover new knowledge, that the best science results from bringing together the best minds, and that the best people prefer to have minds even better and more creative than their own around them. Therefore we have created and continue to nurture a special climate in which excellence and freedom of thought, the third "F," are the dominant values.

Intellectual freedom combined with scientific challenge helps Bell Laboratories attract the most intelligent and creative scientists and engineers. Another major factor in attracting talented researchers to Bell Laboratories is the opportunity to interact, on a daily basis, not only with other researchers but also with development and system engineers and with end-users.

While overall focus is provided by management, it is the individual scientists themselves who set and evaluate specific priorities for research. Those closest to the scientific work have the most intimate knowledge about that work: what is possible, what is not possible, and why. Therefore, our scientists are free from arbitrary limitations on what they can or cannot do. In addition, they are largely free from day-to-day problem-solving tasks and from short-term pressure to produce results. On the other hand, they are highly motivated to pursue the fundamental work that may involve sustained effort over long periods of time.

Specific research priorities are set at the local level because we at Bell Laboratories strongly believe that the future cannot be predicted from an extrapolation of the present. If specific priorities were left to managers with responsibility for developing and exploiting the resulting technologies, it is likely that research, although improving existing technologies, would not lead to the discovery of radically new principles, processes, or devices. Defining specific problems for mature researchers is likely not only to be counterproductive, but also to damage the creative environment necessary for new discoveries. Creative minds need the freedom to roam within a wide intellectual space. This space is an arena of awareness of the overall mission of Bell Laboratories and the Bell System -- a pervasive awareness that has guided our management style and philosophy in research.

An important focus for our research work is provided by a free flow of information and people between our research area and our development and engineering areas. Significant numbers of our research scientists move into our development organizations, providing a tangible connection between research and development. In addition, the direction of research is influenced by the problems and opportunities our researchers see through their contact and involvement with the operating entities of our business. Our scientists benefit from successful exploitation of their work in providing solutions to the real-world problems of the Bell Operating Companies and Long Lines, our embedded businesses.

Let me cite a couple of illustrative examples to show how the combination of focus and freedom has contributed to breakthrough discoveries and inventions. The best known example is the invention of the transistor in 1947 -- the invention that created today's solid state electronics industry, with worldwide sales estimated at $10 billion annually. Since you have already heard the transistor story many times, I will resist the temptation to tell it again. Instead I will take as my first example the development of the UNIX® computer operating system.

As the price and performance of large-scale integrated circuits have improved spectacularly over the past 20 years, computers and their associated software have grown more and more complex. In the early 1970s two Bell Laboratories scientists helped reverse this trend by developing a powerful computer operating system (the set of "housekeeping" programs that organize and schedule a computer's activities) that emphasizes simplicity and user convenience. Recognizing that many of the human-engineering advances in large-scale operating systems could be provided more efficiently on smaller "minicomputers," the scientists developed the first version of the UNIX system on a little-used minicomputer at Bell Laboratories. The system is composed of small, readily understandable pieces that can be put together almost like "tinker toys." It has the additional advantage of "portability"--the ability to be used on different types of computers. The system was first used for word-processing in our Patent Department, and later achieved a very broad range of capabilities, including software development, support of test and development systems, and administration of large databases such as those used for telephone company operations and record-keeping. Because of its ease of use, the UNIX system is now being used at more than 2,000 sites and is supported by over 100 commercial suppliers. It is being used on a variety of machines, from IBM, DEC, UNIVAC, Amdahl, and Interdata to Western Electric's 3B-20 and to the Bellmac-32A single-chip microprocessor. The important point about this development is that while management stated the objectives and provided experience, resources, and encouragement, it did not set detailed priorities. The UNIX system was the result of a free flow of ideas and information throughout Bell Laboratories.

Another example of the value of allowing those closest to the actual work to set specific priorities within a broad focus comes from my own personal experience. Following the invention of the laser at Bell Laboratories in the early 1960's, it was difficult to predict exactly which types of lasers ultimately would become useful commercially. To some, however, it seemed that solid-state lasers were the best prospect for high-power industrial applications such as cutting and drilling. There appeared to be sound theoretical grounds for this view, and crystal-based solid-state lasers already had been demonstrated at higher power levels than the competing gas lasers. So it was very tempting for management to drop all work on gas lasers, and put those resources into development of known high-power lasers. Fortunately, such a decision was not made, and I was free for several years to pursue my work on gas lasers. This work eventually led to the carbon dioxide laser. Today, the gas laser which I invented is the only type useful in high-power applications. It is currently used in industry for cutting, welding and drilling, and in medicine it is used in a wide variety of surgical procedures as a replacement for conventional scalpels. Its current sales, not including defense applications, are about $100 million annually.

With these examples, I have tried to show that the combination of sustained funding, focus and freedom is a synergistic combination; that each benefits from the others. We at Bell Laboratories have been fortunate to have all three operating in the same place at the same time.

Over the years AT&T's investment in Bell Labs research has paid off handsomely. In addition to the discoveries mentioned earlier, Bell Labs produces an average of more than one patent every working day, and now holds a total of almost 20,000 patents, including many of the most fundamental patents in the physical and computer sciences. Through our long-standing and continuing policy of patent licensing and open publication of scientific results, the same knowledge base that has produced so many patents also has brought considerable and wide-ranging benefits to our nation as a whole.

Thus the priority-setting process I have described has worked well for Bell Laboratories, although we have no proof that it is ideal. But just as we believe that research is an ongoing process, so the priority-setting is an ongoing process. We constantly seek to improve the system, keeping in mind our overall goals while modifying the system to improve the efficiency and cost-effectiveness of the research enterprise.

I shall now address the issue of applying what has worked at Bell Laboratories to Federal science priorities. It is important for the members of this Committee to recognize that scientific research succeeds best when excellence--the merits of the science and the scientists themselves--is the primary criterion for priority-setting. This requires that scientific priorities be set by scientists who are held accountable for their decisions. The setting of scientific priorities by those who either do not sufficiently understand the process of research or are not accountable to the scientific community is rarely successful and is often counter-productive. For example, attempts by governments to legislate a timetable for scientific and technological progress have almost always failed. Scientific discoveries cannot be legislated. Management of scientific research requires a deep knowledge of science. It must proceed by objective, not by directive.

A distinction should be drawn between scientific and technological progress. In development of technology based on known science, it may be appropriate to set goals through a legislative process of consultation, compromise, and consensus. The Japanese have demonstrated this through their various multi-year plans. But when the goal is basic scientific research--the discovery of new knowledge--budget-makers must rely on the expertise of scientists to set priorities effectively. Federal policies should be limited to setting objectives within broad fields of scientific endeavor, such as solid-state physics or genetic engineering. Beyond the provision of such broad focus, the scientists and scientific managers should take over. In addition, the consensus aspect of Federal decision-making, with its multiplicity of constituencies, is likely to lead to a wide dispersion of scientists and scientific facilities. This works against the principle that outstanding scientists attract other outstanding scientists, creating a synergy that produces the best science.

Setting aside these reservations, I believe that many of the fundamental principles underlying the priority setting process for research at Bell Laboratories are applicable to the Federal support of science. Among these principles are:

---that the best scientific research requires sustained funding,

---that overall goals of research enterprise have to be well understood and be accepted by researchers as well as managers,

---that the individuals involved in priority-setting make those decisions they are best qualified to make,

---and that no matter how large an organization or a research effort, new knowledge is created by individual scientists.

Above all, we need to recognize that new knowledge and new technology cannot be created by legislation or fiat, and that no amount of planning or priority-setting can replace human curiosity, ingenuity and creativity.

Mr. SHAMANSKY. Thank you, very much, Dr. Patel.

Would Dr. Paul be kind enough to return to the witness table so we can ask you as a panel, if I may exercise the Chair's prerogative.

It seems to me that I underlined in Dr. Paul's testimony on page 5—you were commenting, Dr. Paul—that because of the increase in regulatory activity, a lot of the research was done toward that kind of a thing.

Now you are saying, "An increasing share is now aimed at new product and process development and at productivity improvements. These trends and supportive government policies can become major contributors to economic recovery."

Apropos that I can't forget an article that appeared—I think it was the lead article in the Sunday Times Magazine of a few weeks ago—about robots, I think, showing that General Motors is now having a contract with a Japanese company which developed the robots that GM developed in the first place and didn't pay attention to and now is coming back to work with them.

My question is, do you have any views, having worked with as many companies as you do, because you work with them on a contract basis, if you have an opinion, how did our American companies get into this attitude in the first place of not utilizing the things developed, say, at Bell or that GM itself developed in the first place and said we are not in the business of robotics, we are in the business of making cars and then walked away from it?

Dr. PAUL. I won't presume to speak for Bell Laboratories. Our colleague can speak to that.

I think we are seeing a shift in industry, brought about by great concern for the cost of production and productivity. We are also doing some things for the Government. For example, we are working for the Air Force in developing highly automated manufacturing facilities for reworking turbine blades for jet engines, where there is a great opportunity now for us to have a synergism between what we are doing for the Government, the development and know-how there, and the transfer into the industrial sector.

Your question of how we got into that situation has involved in it perhaps a complacency, perhaps a lack of sufficient competitive spirit at one point in our industrial attitude, perhaps the belief that we could perhaps bull our way through things without utilizing the more sophisticated technology, and other countries jumped ahead of us.

I think we have to catch up now. There are a number of things happening that have exhibited an intent to catch up and again exceed others.

Mr. SHAMANSKY. Do you have the feeling that there is a changed attitude on the part of the people who come to you for work to be done by you?

Dr. PAUL. Yes. We quite clearly see that in industrial sponsorship of work, a very marked increased of concern for the productivity of their whole manufacturing operation, I would say.

Mr. SHAMANSKY. Dr. Patel, if I may ask you, your whole system, as a result of the consent decree that has been worked out between the Federal Government and the Department of Justice and your parent company, augurs some kind of a change, it seems to me, for Bell Laboratories.

Historically you could go along and do the abstract work as required because as part of the whole Bell System, your phase was there and they just provided for it and spread the cost around.

Isn't there a concern now that Bell Laboratories is going to have to be more short-range, more product-oriented as distinguished from a longer range and abstract scientific sort of thing, a pure science approach?

Dr. PATEL. I believe that is a valid concern. However, we must look at the reason why long-range basic research at Bell Laboratories has been supported. It was not supported out of kindness of the heart on the part of our owners. It was supported because AT&T has been convinced that support of such research would result in an eventual payback which will benefit the company.

The past shows that the investment AT&T has made in research at Bell Laboratories has in fact paid off handsomely. In the new environment where AT&T is free to compete in a broader arena, both the management at AT&T and Bell Laboratories firmly believe that if AT&T is to compete effectively in the new arena, the only real leg-up we have on our competitors is in fact our basic research. We must use that as effectively as we can.

The changes that I anticipate will not be in terms of there being a greater attention toward short-range research projects, but toward making sure that the research that is being carried out, and the results of research at Bell Laboratories, are exploited as rapidly and as judiciously as possible, in order to make the process of innovation complete in a shorter period of time.

As you well know, the process of innovation starts at the stage of discovery of new knowledge, goes into inventions, subsequent implementation, manufacturing leading to improved products at lower prices, and better performance.

What I suspect will happen and what I think will happen is that that process of taking things from research into development, into manufacturing, sales and so on, be speeded up.

Mr. **SHAMANSKY**. That will be speeded up, but you don't feel that the lead time, the luxury, shall we say, that you have had heretofore of doing the abstract, the basic research, you think you will still have that luxury?

Dr. **PATEL**. Yes. For the present we have only the words of the management at AT&T to assure us that basic research will continue to flourish at Bell Labs. However, it is also clear that the success AT&T has had in the area of communications, being able to stay ahead, being able to innovate as fast as we have been able to, relies upon the fact that we have our own in-house research ability to find new principles and new processes leading to new devices, new systems, and new services.

I am quite confident that the new consent decree will not affect the amount of research we do. What it will do is inject a new awareness that the job of Bell Laboratories, especially the research area, does not end by publishing a paper, but goes beyond. We have to make sure that appropriate parts of company are aware of what has happened and appropriate parts of the company pick up those results to make new devices, systems, and services possible.

Mr. **SHAMANSKY**. If you would permit me to observe that that may well make change, what is going to happen—I am not hoping that it does, but I think there is a danger that the awareness would not come on the part of those who are supposed to pick it up after you are done but may be on the part of your own scientists, to make them more immediately product-oriented.

That is a danger inherent. I am not asking you for a final answer. It just seems to me that that is implicit in the situation.

Dr. **PATEL**. If you don't mind, let me make a comment on that. The key point is that there has been a separation and there is a separation between the sources of funding and those who provide the focus for our basic research. This is the key feature that will assure us that we do not get sucked in, so to say, into doing those things which bring us immediate rewards, because it is the responsibility of the managers of science to make sure that AT&T will be a viable institution because of its technological ability to compete effectively, not only today, not only tomorrow, but also 10 and 20 years from now.

Mr. **SHAMANSKY**. Thank you, Dr. Patel.

The chairman is back now. I will relinquish the Chair to Congressman Gore.

Mr. **GORE**. The gentleman from New Mexico?

Mr. **SKEEN**. Thank you, Mr. Chairman.

I would like to ask Dr. Paul, there is a very interesting statement in the transcript that you spoke of that gave me pause for thought, on the top of page 3, that you have a responsibility and then further on down that one of the responsibilities of the scientist is to courageously shut down undertakings when they have outlived their scientific usefulness.

As an observer of what goes on in a lot of laboratories, science is one thing and politics is another, if we can get them together. Many times it seems to me that scientific efforts take on a politics of their own.

We spent an awful lot of time and we spent an awful lot of money throughout the years in pursuing scientific pursuit that we should have had the courage to shut down at one point or another but because they have a political constituency they seem to live on and on.

Coupled with this statement, over on the other page, is the level of general knowledge the general public has because anything that happens to the scientific world is reported through the media and it goes through a political process as well.

Is this a large problem, the ability to—I would think it would be less of a problem in a private laboratory than it is in one of the national labs. Could you comment on that kind of comparison and how large the problem is?

Dr. PAUL. I think in any laboratory courage is required on the part of management to shut down any project that is not going well because generally it is associated with good people. These people have a lot of their career invested in the enterprise and a lot of emotion involved. Sometimes the judgments are very difficult to make.

It is not quite clear that success isn't going to come next week, because in doing research and some of the earlier development work, by definition that is the exploration of the unknown.

The people concerned usually are optimistic. They say, just one more experiment and it is going to work. But you reach a point where there has to be an intervention by parties who are not that closely involved with it in making some judgment.

Really, I think courage is the right word, taking the action to say well, that is enough, we have to put our resources somewhere else.

What was the thrust of the second part of your question, may I ask, again?

Mr. SKEEN. How large a problem it is as far as the resources available to you from the Federal Government; for instance, its involvement—and by the way, I am a little disappointed. I see the National Science Foundation now is not making the distinction it was at one time between basic and applied research because it was sort of hard to draw the line.

I guess maybe they are like we are. We don't like to make political decisions, either. The Science Foundation has had the same problem. Just how large a problem is this?

Dr. PAUL. Part of your question I think concerned the understanding of the general public about science and technology, whether that is a difficulty. I really believe it is because there are a lot of apprehensions about science that are just completely unfounded on fact.

We see that very severely in our responsibility for the Government for the Office of Nuclear Waste Isolation; that is, where are we going to put this waste, wherever, underground. I think with very, very few exceptions the scientific and technical community believes those problems are either solved or are very solvable.

There are great social problems, economic problems, and political problems in coming to a resolution on that problem. I think for us as an institution, this is a case where we are learning and we are contributing in a very new way to how do you work these various forces together, the technical and the economic and the social and the political, and get an understanding that is based on some kind of verifiable facts rather than just myths and apprehensions.

So, it is a real problem, and I think part of it rests on the general scientific education of our population as compared to maybe the Japanese and some of the European countries.

Mr. SKEEN. Is the news media in the United States an effective method of disseminating knowledge? Do they have the time, do they take the time, in your opinion?

Dr. PAUL. I think some do a very good job; others certainly don't. I think others latch on to advocacy positions rather than the science.

Mr. SKEEN. Thank you very much, Doctor.

Thank you, Mr. Chairman.

Mr. GORE. Thank you.

The gentleman from Pennsylvania, the ranking member of the Investigations Subcommittee, Mr. Walker.

Mr. WALKER. Thank you, Mr. Chairman.

Dr. Patel, you stated that research management allocates resources in response to opportunities identified by the scientists so that your priorities are targeted from the bottom up, so to speak, rather than from specific directions from your upper management.

Dr. PATEL. That is correct.

Mr. WALKER. In light of that, I wonder what your feeling is about the habit of Congress to mandate particular kinds of research, sometimes based upon exactly what Dr. Paul was talking about of myth and advocacy.

How does that affect research?

Dr. PATEL. The first thing we have to recognize is that the opportunities that scientists identify at Bell Laboratories and the ones which are evaluated, happen to be in a broad range of fields which are of interest to us.

That is where we do have focus. Focus comes from the fact that we generally must assure ourselves that the technology of 10 or 20 years from now will have the right kind of scientific base and scientific underpinning. Areas such as semiconductor physics, solid-state physics, computer physics, chemistry, and so on, are the kinds of areas in which we are broadly involved.

If the Federal funding mechanisms in fact direct moneys toward research in a broad range of areas rather than saying go discover me a new principle of levitation, for example, I think that kind of priority setting is likely to work. But what is needed, simultaneously, is a mechanism for online, ongoing evaluation of what is going on.

One point I made was that in research, the process of arriving at results is just as important as the final results, and sometimes even more so, because very often when you start a research project, you don't know what you are going to find. You have rough ideas of where you want to go, but you don't know what specifically you are going to find. You don't even know if you are going to find something.

In that case, it is very important that the process of research be very well documented so that we know, when everything is said and done, what not to do next time around. This is a very important part of evaluating the bottomsup priority identification process that goes on, something that is usually difficult unless there is a mechanism whereby you have identified a given area of focus which you want to influence.

Mr. **Walker**. Do you generally find that where congressional mandates derive research, that what we are mandating would be what the laboratories choose to do anyway?

Dr. **Patel**. That is a difficult question to answer. However, I think one must distinguish between research and development. I suspect in the research area the way the National Science Foundation, for example, has supported basic research is quite reasonable because there the funding is given to a specific area to further science, to gather new knowledge in that area, rather than doing a specific thing.

The moment you put down bounds as to what you want back from the research with any kind of specificity, it is no longer research, it is development. You are asking questions not as to if it is possible. You have already predetermined yes, it is possible, go find me that.

Mr. **Walker**. Do you see that as a problem in the way in which Congress mandates funding at the present time, that we do generally tend to say this is what we want, now go find a way to get there?

Dr. **Patel**. I believe it is a problem because it puts rather different kinds of pressures on individuals. Very often in research we find that unexpected results turn out to be more important than what you initially set out to find, anyway.

There is no mechanism generally to take advantage of these unexpected results at the Federal level because there is no strong coupling between the research and its further utilization to complete the chain of innovation.

If the second part existed, then of course unexpected results will very quickly get utilized and we will have the kind of synergy that we have seen at Bell Laboratories, where research, development, and manufacturing are closely linked together to provide feedback, which is a very important part of any industrial enterprise.

Mr. **Walker**. The other side of it from our perspective, and of course this is one of the difficult political issues, is how do we receive some assurance that when we are putting money into research where we don't have some specificity, that in fact we are getting something for our money?

How are we provided with the assurance that we are not giving money to somebody who is performing a lot of research but is basically incompetent, is never going to get anywhere and never going to get us results that are not only useful but are not going to get us the ancillary results, either?

Dr. **Patel**. Therein lies the importance of an ongoing evaluation process. Having scientifically trained managers, managers who have been outstanding scientists on their own at one time and now are managing, is in fact an important part of evaluating our research.

Mr. **Walker**. That gets back to Dr. Paul's point that we have to have somebody with the courage to stop that kind of research from being funded where we find that that is what is happening.

Dr. **Patel**. That is absolutely correct. I think in our environment on the whole we turn over a project every 5 years to make sure that even when the individual is producing, we do not wish that individual to fall into a rut, because the moment you fall into a rut your eyes could be closed toward new things that ought to be happening in that given field of enterprise.

Somebody else might pick up the project, but there should be continual change, to make sure that there is a constant infusion of new ideas and new people into that particular branch of science.

Mr. **Walker**. Dr. Paul, do you have any comments on the whole business of congressional mandates?

Dr. **Paul**. Several. I will take one of your last ones first, regarding how do you make the judgment to shut down something.

I would like to come back to my comments about the importance of the Government having scientific competence itself. It isn't the numbers of scientists and engineers in the Government, it is their quality that is important. If the Government is going to disperse funds for research and development, it should do it out of a real base of authoritative knowledge and not have agencies that could be snowed by incompetent performers, your point.

Mr. **Walker**. I guess one of my concerns, though, is that the agencies may have those people but there is some tendency on the part of Congress not to listen to the agencies sometimes. You don't necessarily have a degree of scientific expertise that sits, for instance, on these panels, known as the Science and Technology Committee, where we make those judgments. We have very few scientists on it.

I have told many people in speeches that I have got some high school science teachers and some college science professors who are either rolling over in their graves or pulling their hair out thinking I am having anything at all to do with the science policy of the Nation.

You know, you do have people who are making judgments who are not necessarily skilled in those areas, and yet we are mandating things to some of those highly qualified professionals that we have in the agencies.

That is where my concern comes, and that is what I am looking for there, is to what kind of havoc that wreaks in the science community as a whole.

Dr. **Paul**. I agree if there is a problem we ought to find a way to fix that problem; collectively find a way.

The second part of your question about Congress mandating, I speak out of my own experience and observation that when the nation has held out grand goals that require scientific and technical achievement, somehow we respond to them.

My own experience has been primarily in atomic energy one way or another. In the early fifties we had a national policy of nuclear deterrence. Those of us who were working within the system in how to make weapons materiel had a very clear and compelling mission to make more, more safely at lower cost. A lot of good science was stimulated back then.

In the midfifties there was a peacetime atomic energy program. I think the scientific community just responded in a grand manner. All kinds of things came out: The applications in medicine, and in agriculture, and industrial testing and gauging, which still exist and are largely unseen. All the attention now is diverted to the nuclear powerplants, which were a part of it, too.

In the sixties there was the space program. Many good things came out of that. As Dr. Patel said, you can't foresee in advance where those things are going to lead. They capture the imagination and people relate their own ideas to those grand goals.

In the early seventies I think we had an opportunity, but it kind of fizzled, and that was the whole energy business, to really make this country more independent in energy. Some good things started to happen. In our institution we tried to do things, and it just kind of went a little flat.

It is very important I think that in every period of our society there be a few of those grand goals that call on the scientists and the engineers to respond. I think there is a role for Congress in that, to conceive and support those kinds of things.

Mr. WALKER. I have just been reading recently some people who are suggesting, some people from the scientific community and some from the futurist community, who are suggesting that the high frontier, the high road is exactly the kind of goal that we ought to outline for the nation because that moves to that kind of thinking. Would you tend to agree?

Dr. PAUL. I certainly would.

Mr. WALKER. Thank you, Mr. Chairman.

Mr. GORE. Thank you.

Let me say I also agree we ought to be setting some imaginative goals to work toward. I think we missed the boat in abandoning some of those goals.

You, Dr. Paul, have this unique corporation that doesn't have any stockholders.

Dr. PAUL. Correct.

Mr. GORE. How does the absence of stockholders make your priority setting much easier, or does it?

Dr. PAUL. We say that gives us both the right and the obligation to be independent and objective in our contribution to science and technology. We should not be beholden to some party for other than scientific and technical reasons.

In our own internal process of selecting what we work on, we refer you again to the idea of internal research and development. To simplify it, we can think of it in our organization as three levels. I think it coincides quite closely with what Dr. Patel described.

At the smaller working level, in our case the research and development department, that may be 100 to 200 people. The manager of that department has limited discretionary funds at his or her disposal. They use those funds generally in a time span of a few months to 1 or 2 years, looking out. It is a fairly safe kind of thing to explore.

At the next level of management, which is the laboratory, and that may be 500 to 3,000 people, there is discretionary money that is controlled by a process that includes the inputs from below but also that higher management judgment. In this system they are looking at several years to maybe 4 or 5 years out.

At the institute level, our total corporation, we have about 1 percent of our total activities that we stimulate with discretionary money. We are trying to look at three to ten years out.

You can't really apply strict cost benefit analysis to that for the reasons that have been discussed. If this is research and exploration, you don't know where it is going to go, the payoffs may be completely unforeseeable, you are operating at a considerable extent on faith in particular people and faith in the soundness of particular ideas. You place your investments in them and then you try to stay with them long enough to see whether your faith is justified.

I think it is a mistake for any institution to try to take a basic research investment today and tie it to some economic payoff 5 or 10 years down the stream. Those things diffuse, disperse, and payback comes in many different ways.

Mr. GORE. Dr. Patel, on the same question, your parent corporation of course has more stockholders than any other, if I am not mistaken. You seem to be attributing your remarkable success in part to the fact that there is a clear separation between your accounting department and your scientific research choices.

Is that correct?

Dr. PATEL. That is correct at a microlevel. It is not correct at the macrolevel because in a definite sense we have a mission. The research at Bell Laboratories does have a mission, and that is to support the technology that has been chosen by our parent institution as being the right one for products, services, or whatever may be the case. This mission provides the broad focus for the Bell Labs research.

Within this area of broad focus, the mission of say for example solid-state physics, chemistry, computer sciences, the choices of which directions to take, what projects to support, where we think there are going to be payoffs, those kinds of decisions are left to technical management at Bell Laboratories.

As Dr. Paul mentioned, we can justify the correctness of our decisions only many years after something has already happened by saying something has worked. I think our record in a sense speaks for itself. The long-term investment has paid off, and it has paid off handsomely.

As long as we continue to have excellent people and good managers, I think we will be able to assure our stockholders and our parent organization that the money that they put into us will be well spent and the knowledge that is gathered from that money will create and support the new technology that will be here ten years from now.

Mr. GORE. As both of you may know, your appearance here today is the last part of a much longer hearing that started earlier. We had originally wanted you to come then, and I think our schedule—I forget how it developed—but I want to ask you to comment on the larger questions that have been addressed by people who have different vantage points on this process, the larger question of how the country selects its research and development priorities.

There is an evident erosion in the public support for the scientific research enterprise, which I think is unfortunate. This administration has said, as I noted in my opening statement, that we can't any longer afford as a country to increase our commitment to research and development, that there has to be a decline. They go further to say that that is OK because it won't hurt the scientific enterprise even if we do reduce our support for it.

I disagree with those two statements very strongly. Do you, Dr. Paul?

Dr. PAUL. I certainly disagree with the idea that the fraction of our national resources, GNP, budget, however you want to characterize it, can decrease without harming our welfare. I think the scientific research and technological development is the wellspring from which our future comes.

Mr. GORE. Well, what about the notion that it ought to fluctuate according to the current economic trends and how much we think we can afford in a given budget year? Do you think that is a shortsighted approach? Do you think that is a leading question?

Dr. PAUL. I hear exactly the same question inside Battelle. When we are having hard times, should we tighten up on our long term research.

I think everybody has to share in the sacrifice, but it should not be cut to the point that we really cannot recover in a reasonable time. The surgery is really a few people, it is individuals, and if you cut them off and they go off and do something else, then they are out of touch, they are not abreast with their counterparts in Russia or Germany or Japan, and we will fall behind.

Mr. SHAMANSKY. I would like to point out to the chairman and to the panel that when we talk about the limits, that times are tough for the Federal Government, that is very much a matter of choice. We seem to have billions for subsidizing things like excess production of milk products. We have billions for tobacco subsidies. I don't want to tread on any toes, Mr. Chairman——

Mr. GORE. You just did.

Mr. SHAMANSKY. I realize that. But we have to be careful when we talk about what we have and what we don't have available. One of the basic decisions is—and every person's interest is special to him, so I am not going to knock special interest per se, but we shouldn't confuse here the idea that this Government, this country doesn't have available funds.

All we are talking about is the allocation of the funds and the importance we put on them because we have billions that we are simply throwing to certain classes of people to maintain them in programs that got established 50 years ago in the depression and are simply just rolling along, no changes at all.

Mr. GORE. Dr. Patel, did you want to comment?

Dr. PATEL. Yes. I think what we as a nation face here in the next 10 years or so is a real challenge from our industrial overseas partners and competitors: Japan and Europe. If you want to stay ahead in technology, we have got to stay also ahead in R&D that supports it.

In my opinion, there are techniques by which one can speed up the process of use of R&D and make it pay off faster than what we as a Nation do. We have not fallen behind in science and research and development. The place where we are falling behind is in the coupling of R&D to industrial innovation.

That is where the real stress has to be put, to make sure that we don't sever that link between research, development and industrial innovation. What I see happening is that for reasons which are all too obvious to lots of people, much of our industrial innovation is incremental in nature. There is a real difference between incremental improvement in a product and innovation leading to radically new products.

It is the latter one that we are falling behind in because as a nation we are not taking advantage of the R&D that we have. It requires encouragement of investment in new technologies.

Mr. GORE. The example of the video tape recorder I think is an appropriate one to illustrate your point. That business is being dominated by the Japanese. In fact, both here in the United States and Europe we are witnessing a huge surplus and dramatic price reduction on the first generation machines.

The technology was really invented here in the United States. Yet, we did not take it to the market first. I think you are right in pointing to that link. That is something that Bell Labs has been so good at.

Do you agree with those who say that one of the main reasons why you have had such success is that you do have sort of a creative tension between product-oriented or process-oriented research and basic research; in other words, you have a broad mission but that serves the purpose of giving you a goal to aim toward, yet you also have the freedom to humor those scientists who want to take a tact that looks like it has no relevance to the broad mission at all in its early stages?

Dr. PATEL. I think that the phrase creative tension is a good one. It is a creative tension that is brought about by a day-to-day interaction, a day-to-day interaction which leads to respect for the two parts of our enterprise: those who create knowledge and those who apply knowledge. Often, these are the same people. It is precisely that day-to-day interaction in terms of free flow of ideas, free flow of information, and very importantly, free flow of people back and forth between research and development, which facilitates the conversion of ideas into technology.

Mr. GORE. And the better setting of priorities.

Dr. PATEL. And leads to better setting of priorities.

Mr. GORE. Maybe we can figure out a way to get that kind of creative tension working for us in other aspects of the nation's research enterprises.

Let me thank both of you for spending the morning with us and contributing as well as you have done to these hearings. I say again that you are the last two witnesses in a more lengthy proceeding which started before the October break We appreciate your appearance here and again thank you.

With that, the subcommittees will stand adjourned.

[Whereupon, at 12:03 p.m. the subcommittees adjourned.]

APPENDIX I

THE CONCEPT OF SCIENTIFIC CHOICE: A BRIEF REVIEW OF THE LITERATURE

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I

Problems of choice among different allocations of scientific effort have become a subject of concern to all societies. The developing nations face a series of urgent decisions about what kinds of research effort and educational institutions to support in order to stimulate a rapid rate of social and economic development. The industrialized nations, particularly as they reach the stage of "big science,"[1] have found that difficult choices arise with respect to allocation of resources among different branches of scientific activity and also in determining the extent of society's support for science relative to other desirable social activities. The scientific community can no longer be regarded as an autonomous enclave comfortably insulated from the larger social context. Governments have a vital stake in the progress of science and technology and, like other activities that involve substantial public expenditures, science and technology must operate within some general framework of public accountability.[2]

The pressing need to make discerning use of scientific and technological resources has given rise to a series of efforts to define the concept of "scientific choice." The purpose of this paper is to present a brief review of that literature and to suggest some possible lines for future research.

II

Alvin M. Weinberg's two articles on "Criteria for Scientific Choice" provide a convenient starting point for the discussion.[3] Although not the first to write on the subject, Weinberg has presented one of the most comprehensive approaches to questions of scientific choice and his articles have done much to give the debate the widespread interest it now enjoys. In his first article, Weinberg begins by noting that the current era of rapid expansion in science budgets is coming to an end. Support for science has reached the point where it is politically visible and where there will be pressures to rationalize and justify the pattern of expenditures. Whereas in a period of rapid growth choice could be more easily avoided, choice has become essential in a period of critical scrutiny of science budgets and growing pressures on available manpower resources. "As a practical matter," he concludes, "we cannot really evade the problem of scientific choice. If those engaged in science do not make choices, they will be made anyhow by the Congressional Appropriations Committees and by the Bureau of the Budget, or corresponding bodies in other governments."[4]

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Thus the problem becomes one of formulating some scale of values which might help establish priorities among scientific fields so that important choices are not left solely to political bargaining and maneuver. The most difficult and important choices, he recognizes, will often involve incommensurables -- choices among distinct fields of activity "whose only common characteristic is that they all derive support from the government."[5] Accordingly, he proposes three sets of criteria to assist choice on these difficult questions. The criteria are "scientific merit," "technological merit," and "social merit." "Technological merit" is fairly straightforward, referring mainly to a balance between research costs and prospective returns which society could hope to realize from investment in various fields. The "scientific merit" concept is more difficult, having to do principally with a field's "ripeness" and intrinsic scientific promise. An interesting feature of Weinberg's "scientific merit" concept, however, is the suggestion that merit rests importantly on the extent to which a field contributes to its neighboring scientific disciplines as well as on its intrinsic promise. "Social merit" is a broad and elusive concept which concerns such political and value considerations as national prestige, health, welfare, and military power. Weinberg proceeds to examine the worth of five research fields using these three sets of criteria. A fundamental assumption is that a field should rate highly on more than one set of criteria to deserve large-scale public support. In his view, molecular biology rates high in all three respects, high-energy physics is perhaps somewhat overrated, and space research has little merit by any standard.

In his second article, "Criteria for Scientific Choice II: The Two Cultures," Weinberg sets himself the broader question: what criteria can society use in deciding how much it can allocate to science as a whole rather than to competing activities such as education, social security, foreign aid and the like? The first step, Weinberg argues, is to reject the notion of "science as a whole" as a misleading idea. For budgetary purposes, there should be a clear separation between basic science and that science which is supported as a means to further other ends desired by society. In this way, much of what is now regarded as support for science could be more sensibly considered as competing for resources with other activities that might further desired social objectives. For example, suppose we wished to control the population of India and had certain resources at our disposal for that end. A number of possibilities are open: supporting research of various kinds, buying and distributing contraceptive devices, arranging for some monetary incentive to induce Indian women to use birth control, and so on. The various means would be evaluated in terms of their likely contribution to the goal of population control; if research were decided on, it should be reckoned as an overhead cost incurred in the process of achieving population control. Thus "the scientific work that goes toward solving this problem ought to compete for money with alternative, non-scientific means of controlling the growth of population in India rather than with the study of, say, the genetic code."[6] Weinberg would caution against "overselling" the potential contribution of scientific research to the solution of practical problems. He would also like to see government agencies continue to support a good deal of mission-oriented basic research (if for no other reason than to enhance the productivity of the applied research). But the separation for budgetary purpose of basic research from applied research and mission-oriented basic research would have one overriding advantage: it would drastically reduce the size of the science budget (and hence, presumably, reduce the political "visibility" of massive public support for science).

However, this still leaves the knotty question of what rationale can be used to justify large-scale public support of "pure" scientific research which cannot be tied to any practical end. First Weinberg examines the alternative of a cultural-aesthetic justification, which leads him to consider the analogy between support for scientific and for artistic activity. He finds this in some ways a potentially fruitful approach. Pure science and art are alike in many ways and in the value they provide society; and a plausible case might be made for greater public support for science since art receives a great deal of private support which is unavailable to science. But he rejects this approach in favor of the more practical alternative of justifying basic science as an overhead charge on the society's entire technical enterprise -- an assessment which seems appropriate "because, in a general and indirect sort of way, such basic science is expected eventually to contribute to the technological system as a whole."[7] Until and unless society acquires the sophistication to appreciate science in its own right, Weinberg remains skeptical that the aesthetic justification can provide a basis for support at the level which would be in both science's and society's best interests. Thus in Weinberg's approach applied science is regarded as an overhead charge on the tasks it seeks to further; mission-related basic science is considered an overhead charge on the mission-related applied science; and pure science can be viewed as a necessary overhead or social capital cost on the entire technological system.

Weinberg's approach is notable for its understanding of political realities and recognition that non-scientific considerations must legitimately enter into important decisions on society's support for science. He was in part reacting against certain tendencies in the scientific community to consider scientific affairs the exclusive province of scientists.

Earlier, for example, Michael Polanyi put forth a vision of the Republic of Science wherein the interactions among the members of the brotherhood result in more deeply rational choices than could any conscious effort to direct or guide science from the outside.[8] Scientific affairs, in Polanyi's view, are best left to the scientists themselves and to the informal workings of the scientific community. For "any attempt at guiding scientific research toward a purpose other than its own is an attempt to deflect it from the advancement of science" -- and an attempt that is bound to be self-defeating, since science "can advance only by essentially unpredictable steps.... You can kill or mutilate the advance of science, you cannot shape it."[9] Polanyi cites the failure of several British efforts to guide or co-ordinate university science during the past several decades as historical evidence confirming his thesis. Although the Polanyi and Weinberg positions differ rather sharply in important respects, the two are alike in generally approaching the problem with a view of specifying some set of principles or standard of judgment that would provide a comprehensive framework for considering questions of scientific choice. They tend to assume, in other words, that there is one big problem of scientific choice; the challenge is, then, to devise the right principles or procedures to serve as a general guide to choice.

John Maddox's "Choice and the Scientific Community" follows the general approach of Polanyi, but he takes issue with Polanyi on several key points.[10] He does not think of science as inevitably consisting of a decentralized and fragmented network of sub-disciplines with fruitful communication possible only between scientists in neighboring fields. He suggests that "it is possible to think of planning what may be described as a balanced strategy for the encouragement of science," and issues a call for more public discussions and "intellectual confrontations" among scientists to argue out issues and reach decisions on questions of scientific allocation."[11] Maddox reviews some of the historical evidence on the use of committees to encourage support of neglected scientific fields in Britain, and emerges with a much more favorable view than Polanyi's of the usefulness of such a device. The use of "a great many more ad hoc committees" may be an important way to reach "a balanced judgment on questions like the importance that should be attached, in a country like Britain, to high-energy nuclear physics, space research, or oceanography."[12]

Along rather different lines, C. F. Carter in his "The Distribution of Scientific Effort" proposes an economic criterion to guide scientific choice.[13] Writing from the background of an economist, Carter suggests that a useful way to approach the problem is to pose the question: what kind of distribution of scientific effort will most effectively increase the flow of wealth? In turn, this broad question can be broken down into more manageable sub-questions which can be analyzed with some precision and which reflect a country's peculiar problems. In Britain's case, the major need is to expand exports (or to find substitutes for imports). "The examination of what we know about the pattern of exports two or twenty years ahead," then, can provide "a starting point for scientific policy.... By asking these questions about the long-term prospects of British exports (and about substitution for imports) it would, I think, be possible to reach some conclusions, first about applied research and development, then about the pure research which feeds into it, and finally about the forms of training needed to support this research."[14] Carter's approach clearly presents some sharp contrasts with the views advanced by Weinberg, Polanyi, and Maddox. Despite differences in orientation, the other authors accept the well-being and advance of science itself as a basic "given" which must bulk large in any approach to scientific choice. Although he recognizes that any nation as a matter of policy may wish "to undertake pure research beyond what can be given a broad economic justification by its ultimate application,"[15] Carter generally reverses the emphasis and evaluates scientific efforts in terms of their contribution to a nation's economic welfare. Again, however, there is with Carter concern for discovering a general formula to provide some orderly way to evaluate scientific choices.

III

These studies have all added useful insights into the difficult conceptual problems in questions of scientific choice. Some of the important issues have been clarified, and there is a vastly more sophisticated basis for discussion. In a sense, however, these studies may have attempted to do too much. In structuring the problem of scientific choice as though it could be "solved" by discovering the right approach, they have oversimplified the alternatives and failed to separate out aspects of the problem that are logically distinct.

Weinberg, for example, in his first article has not attempted to specify what sort of "trade-offs" might be involved among his respective criteria. Further, his concept of social merit is so general as to be non-operational. In fairness, it must be admitted that it is difficult to part with this elusive social merit concept, unsatisfactory though it may be. Something like broad social merit perhaps must figure in various issues of scientific choice. But it seems appropriate to ask that the constituent parts of "social merit" (i.e., national prestige, power, etc.) be spelled out and analyzed in greater detail,[16] and an effort made to assess what weight should be given to such factors in what kinds of decisions.

Weinberg's second article, though an ingenious effort to make a politically persuasive case for the support of science, might lead to a situation where we were doing the right things for the wrong reasons. It is all right to hide part of society's support for science under a more practical label so long as the mission-oriented agencies take a broad view of what research is relevant to their concerns. But what would happen if they became disillusioned with the role of scientist as wizard and adopted a restrictive definition of relevance? Or if budgetary constraints were to force cuts in the most vulnerable parts of agency budgets? Science fields might suffer the same vagaries of fate as their sponsoring agencies.[17] In striving to take account of political realities, Weinberg may have gone too far in the direction of catering to current irrationalities in official and popular attitudes toward science. He may also have introduced a note of disingenuousness into the debate which could boomerang over the longer run.[18] In the end, there may be no substitute for the sophisticated public understanding of science which Weinberg regards as unlikely to be achieved in the near future.

Carter's economic approach might be helpful in clarifying choices about what sort of applied technology and industrial R&D a nation should foster, but it is clearly less applicable in other areas. Priorities of effort among basic research fields, for example, or decisions on specific projects or individuals to support with basic research funds, are problems which do not lend themselves easily to traditional cost benefit analyses.

Polanyi, on the other hand, may be correct in insisting on the autonomy of science as a community of professionals in certain spheres, and in asserting the community's right to make certain kinds of decisions without outside interference. Judgments on the scientific merit of individuals, the value of research proposals within given basic research fields, and operational questions concerning how research projects are to be carried out are examples of matters that are probably best left to the scientists themselves. Polanyi's guild syndicalist approach, however, breaks down when one leaves the realm of pure science and enters that of applied technology. The kind of debate by which research priorities are argued out within pure science is clearly inadequate as a means for reaching decisions on what lines of technological development to pursue. Inevitably, commercial considerations are involved here. At any rate, it is unrealistic to expect that society will not insist on some voice in areas where expensive developmental activities are financed wholly or in part by public funds. Polanyi's approach in general fails to appreciate that society's chief contact with science is through technology, and that society's ~~general~~ generous support for science is conditioned to a significant degree by the real or imagined technological benefits thought to follow from scientific research.

Maddox's analysis, although he takes certain interesting departures from Polanyi, seems subject to the same general criticisms. Although he believes that it is possible to plan a balanced strategy for science's development, he continues to insist that "decisions on scientific policy" should be "determined by the long-term interests of science itself."[19] Whatever planning is needed should be done by the scientists themselves through intellectual confrontations like those by which scientists argue out the merits of their proposals within the academic world. This is the only way, Maddox suggests, to make wise decisions on the allocation of scientific effort either in technology or pure science.[20] Maddox manifests an uncritical faith in the scientific community's ability to make discerning choices on science policy and, in contrast to Weinberg, seems to have no real conception of the political context within which major decisions involving science must be made. To take obvious examples, society will hardly consider it the scientific community's exclusive province to decide how large the research and development budget will be, the extent of society's support for applied technology as against pure science, the allocation of effort between space exploration, desalination projects, or research on the technology of urban transport. These are in important part value or political decisions, although they involve an understanding of the technical possibilities, and as such accountable officials inevitably must play a prominent role.

Moreover, studies in the sociology of science have established that the "scientific community" does not always behave strictly according to a model of perfect competition in the classical economic sense. There are "diseases" of science, oligarchies and internal power clusters based on age and status distinctions, and personal and institutional vested interests that systematically bias the decision process.[21] Arguments for leaving decisions on science policy to the informal workings of the "scientific community" have, furthermore, always suffered from an inability to attach an operational definition to that concept.[22]

IV

With Stephen Toulmin's "The Complexity of Scientific Choice: A Stocktaking," the debate seems to enter a new phase.[23] Toulmin takes the important step of stressing that there are numerous levels and institutional contexts where choices on scientific effort must be made. Hence it is chimerical to suppose that there is a single big problem of scientific choice. There is inevitably a multiplicity of problems of choice and plurality of criteria to guide choice. "Unless some particular administrative context is specified, questions about 'scientific choice' become essentially indeterminate, for it is the exigencies of the specific context which impose particular criteria of choice."[24] Or again: "...it is the very demand for a single overall 'order of merit' that lands discussions of science policy in confusion."[25] It follows that the quest for a comprehensive "solution" should be abandoned, and our attention should instead be focused on a range of component problems contained within the amorphous general heading of "scientific choice." It is on the more manageable issues that analytical studies might be expected to make an important contribution. In short, what is required is "a clearer and crisper vision of the questions actually at issue in the formulation and administration of a science policy."[26] Toulmin attempts to provide some guidelines by distinguishing four categories of research, and suggests some considerations relevant to choice within each category.

Toulmin's approach clears the air of some troubling problems (by declaring them to be non-problems) and charts a path that could be promising in future research. Unhappily, although he clarifies a number of difficult points, Toulmin adds some confusions of his own. In spelling out the practical implications of his reasoning for the field of public administration, he enunciates a general principle of organization which be believes "holds in the administration of scientific affairs as forcibly as it does in the rest of the public service."[27] This is the principle that the structure of departments and advisory committees "should be so ordered that, at every point, decisions have to be taken between *commensurable alternatives*."[28] In most areas of public administration, Toulmin asserts, this general principle is well-recognized, and "organizational structures have been developed which faithfully reflect and discriminate, the different governmental functions to be performed."[29]

As a statement of historical fact this assertion is surely mistaken. It is a great simplification to suggest that government functions have typically been assigned to organizational units on the basis of some theory of task homogeneity. Department organization reflects more the impact of various "ecological" factors, such as crises, disaster, population trends, economic forces, and the political alignments which grow out of these.[30] As a result, government organization is apt to have a rather disorderly appearance and a dynamic quality that bears little resemblance to notions of abstract logic in the assignment of functions.

More important, Toulmin's view reflects a serious misconception of the nature of the policy process. The decision-maker, except perhaps at the lowest levels of programmed activity, inhabits a *multi-objective universe*. The important choices throughout the administrative hierarchy involve an intricate balancing of conflicting objectives. Rare is the circumstance when choice can be made on the basis of a single clear objective handed down from above. The seamless web of policy formation and administrative action implies a continual refinement of stated objectives and formulation of new objectives. Thus it is misleading to suggest that "in a well-structured administration, choice always involves, say, signing a contract for one fighter-bomber rather than another fighter-bomber...."[31] The choice might well involve buying a transport rather than another fighter-bomber -- or considering whether it is desirable to buy at all -- depending on other objectives being sought through the *instrumental* objective of a fighter-bomber.

Toulmin's premise leads him into some curious blind alleys. He observes, in a tone of genuine surprise, that in the United States the "National Aeronautics and Space Administration has been given both the authority and the means to alter substantially the national division of effort as between (say) industrial development, defense and scientific research. Instead of being a purely administrative agency, it is to that extent a political agency taking political decisions...."[32] The intermingling of policy and administration has become so widely recognized in the public administration literature that this observation seems quaintly archaic. Toulmin goes on to suggest a sharp distinction between "technical" and "political" choices. He implies that the latter should be left to the political authorities, and that scientific advisory committees should consider carefully formulated "technical" questions. Clearly, this is an unrealistically rigid distinction when one considers science's pervasive impact at all levels of modern government.

One is reminded of now discredited notions that used to command some attention in thinking about defense policy issues. "Political" decisions were thought to involve such questions as the overall level of the defense budget and a broad statement of strategic objectives. Then "technical" judgments would determine how to pursue the broad goals most efficiently and purchase the most "defense" for the dollar. This line of thought assumed that generalities like "defense" and "deterrence" were clearcut goals that could be maximized at given budget levels, and ignored the constant interplay between expert and political judgment that often refines stated objectives and generates new policy goals.[33] Toulmin's view, in similar fashion, constructs an artificially high barrier between the realm of policy and that of technical expertise. The view would consign the important choices in science policy to hunch, intuition, value preference, the policymaker's rule-of-thumb assessments -- and would unnecessarily circumscribe the role of technical expertise in clarifying the vital choices. Analytical studies and technical expertise on the most important problems would be discredited as illegitimate intrusions into the pure realm of "value" and "political" decisions inhabited solely by the politicians. Thus in a sense Toulmin's analysis leads us back to the beginning point of the debate. Weinberg had recognized that the really difficult choices in science policy involve incommensurables -- how to allocate resources for science where there are no clear standards of comparison and judgment between different fields of activity. Weinberg (rightly) saw the problem as one of trying to make the bases for decision more explicit, and not leaving important choices on science policy solely to the jockeying of the political process.

In fairness to Toulmin, however, it should be pointed out that he was reacting against certain features of British organization for science which have tended to insulate research activities from effective political direction. Toulmin himself seems a little uncomfortable with his formulation. He goes on to applaud U.S. organization for scientific advice at the White House level -- the President's Science Advisory Committee (PSAC) and the Office of Science and Technology (OST) advisory apparatus. For "their advice serves. not to supplant the ultimate political decisions, but to make them better-informed."[34] Still, Toulmin's analysis only begins to probe the major dimensions of the larger task he outlines: the need for "a clearer and crisper vision of the questions actually at issue in the formulation and administration of a science policy."

V

What would be some salient elements of this "clearer and crisper vision?" First, it seems important to make a distinction between a national policy for science and issues of science in policy. The former involves the development of policies for the management and support of the national scientific enterprise, the strengthening of the country's basic science potential, and the selection and evaluation of substantive scientific programs. The latter refers to political or administrative issues which are influenced strongly by technical considerations, and hence the question arises of the appropriate institutions, procedures, and policies for relating scientific advice to the policy process. This distinction is now fairly well-established in the literature, and it is a useful one provided it is not carried too far.[35] For, as Harvey Brooks points out, an exciting scientific proposal may become an important foreign policy tool and, conversely, a political problem may lead directly to expanded government support for certain areas of scientific research.[36] It is probably also true that science has become a major concern of society, and has received massive public support, only because scientific activities occasionally may have dramatic implications for national policy, especially in such areas as defense, health, transport, communications, and economic growth. Indeed, developments in science and technology provide one of the basic "ecological" factors which give rise to government activity.[37] Thus ultimately the distinction collapses.

Yet taking the distinction between science in policy and policies for the support of science as an analytic point of departure, it may be possible to help clarify some of the important questions actually at issue in the formulation of a science policy. Consider first the role of scientists and technologists in broad policy formation. There have been relatively few detailed studies of the role played by scientists in important policy decisions,[38] and it seems fair to say that there is no generally accepted "theory" on how scientists should figure in the policy process. Some studies have argued that the scientist's influence in the policy process stems from his special status -- his "apolitical" character -- which enables him to introduce new perspectives unfettered by an allegiance to the traditional rules of the political game.[39] Other studies stress that scientists cannot play a responsible or effective role in the policy process unless they recognize that they are in the battle and not above it, and acquire a thorough understanding of the mores of the political system.[40] Some observers seem willing to go farther and to regard the relationship between scientist and policymaker as simply a special case of the familiar problem of expert and politician.[41]

In general, the arguments in favor of scientists participating responsibly in the policy process within the mores of America's pluralist politics appear the more persuasive. But this sort of observation only begins to suggest the wide range of questions that remains to be studied. If scientists participate responsibly in policy formation, does this imply a damaging interruption of the scientific career? What will this do to science? How will government organization be affected by the extensive participation of scientists in policy formation at all levels? If it is true that advising may become a full-time chore, will we have to consider some way to change institutional policies and personal attitudes so that many scientists can expect to serve a "hitch" as advisors without losing their academic (industrial) positions or fatally damaging their careers? What should be the appropriate "mix" between persons with scientific training within the formal government structure and the temporary scientific advisor brought in from the outside? Is it useful to think about defining a more formal code of behavior to govern the selection and use of scientific advisors instead of the present rather ill-defined system of informal understandings? Or would such an effort unwisely limit the policymaker's and the advisor's freedom of action? Are there criteria that can measure the "success" of current scientific advisory practices in any public policy area?

This list, I think, provides a rough approximation of some of the larger questions at issue in the scientist's role as advisor. How might some of these questions be tackled in an operational sense? One area that comes to mind at once as a likely candidate for further research is the work of the President's Science Advisory Committee (PSAC) and its network of committees. Research could build on previous studies of PSAC operations,[42] and might teach us a great deal about what is involved in high-level science advising. As a confidential arm of the President, there will clearly be some limits to what aspects of PSAC operations can become subject to research and open discussion. But it should be possible to learn more about the past membership of PSAC, what branches of science, geographic regions,[43] and institutions they have come from, how they have viewed their PSAC experience, what effect it may have had on their careers, what sorts of problems they have addressed, and their relationship to the traditional departments and operating agencies.

A related topic that might be fruitfully explored is the role that the Office of Science and Technology (OST) has played, since its inception in 1962,[44] in providing advice to Congress. There has been a concern in Congress with the need for adequate scientific advisory facilities.[45] It would be interesting to know more about how that need has been served through the OST mechanism (and other scientific resources of the executive branch). Also, the National Academy of Sciences' role in providing advice to certain congressional committees may warrant study.[46] Due regard should be paid in such inquiry to the likelihood of drastic changes in congressional oversight of executive action if Congress had substantial advisory facilities of its own. There can be little doubt that congressional intervention in the executive decision-making stream would be more persistent, detailed, and pervasive if Congress could tap extensive expertise of its own.[47] This could have important implications for the American governing system, and is an aspect of the problem frequently overlooked by advocates of greatly strengthened congressional staffing and advisory facilities.

In general, on the theory that conceptual discussion and empirical inquiry should go hand in hand, it would seem useful to have many more case studies of how scientists have actually figured in important policy choices. We might then have a better understanding of what qualities and kinds of background contribute to a "successful" advisory role, the strengths and weaknesses of different institutional arrangements, how to avoid unusual conflict-of-interest situations, and other problems of using scarce intellectual resources effectively and responsibly in policy formation. There is some good sense in attempting to define the conditions that make for constructive use of scientific advice in policy formation, but an overconcern with logical neatness or administrative tidiness would miss the essential point. The principal objective of a system of advisory services is not a clear structuring of roles but rather a guarantee that significant problems and policy alternatives are constantly thrown up for the policymaker's consideration. An effective system probably should include advice from a number of different points, some interested and some disinterested, the advice sometimes parochial and sometimes broad in scope, some sources concerned with the long-run and some with the issues of the moment. Thus the need to rationalize, not revolutionize, our present pluralist system of scientific advisory services should perhaps provide the relevant framework to guide further inquiry in this area.

VI

Let us now shift the focus to policies *for* the support of science. We carried the debate above to the point where the quest for "the" right approach to questions of scientific choice began to be called into question. The search for general principles applicable to all questions of society's support for science seemed too ambitious an undertaking. The problem was too amorphous, too beset with conceptual difficulties, too many-faceted for any straightforward solution. A more promising direction for future research seemed to be implied in Toulmin's call for proximate criteria to assist choice in specific well-defined areas. Toulmin began to lay the basis for this sort of analysis by defining certain categories of research support and suggesting considerations that might be relevant to choice within each category.

The most significant effort to date that has sought to carry the analysis of scientific choice beyond the enunciation of general principles is the recent report of a National Academy of Sciences panel, Basic Research and National Goals.[48] The report, done at the request of the House Committee on Science and Astronautics, may represent something of a milestone in the provision of scientific advice to the Congress. The Academy was asked to address itself to two broad questions:

> 1. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural and military applications?
>
> 2. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

The task was delegated to a special Panel on Basic Research and National Goals under the chairmanship of George B. Kistiakowsky. The panel included eight members of the Academy's Committee on Science and Public Policy, plus seven other representatives drawn from government and industrial laboratories and the academic community.[49] The panel chose not to present a unified report based on a consensus of the members' views, but instead presented a collection of independent but supporting papers written by the individual panelists. The result was a much more useful review of the many difficult aspects of the subject than one could expect from a unified committee document.

Most of the papers avoided any attempt to answer the Committee's questions directly in the terms they were posed; the panelists generally confined their attention to aspects of the subject which each felt to have some particular relevance for the larger questions. A common theme sounded by most of the panelists, however, was that the system we have evolved to support science, whatever our understanding of its inner mechanisms, has given the United States a pre-eminence in the scientific world. Drastic changes in the present system, therefore, should be viewed with suspicion. The quest should be for *marginal* adjustments in present policies to assure a continued United States leadership in basic science.

The papers by Blinks, Horsfall, Pfaffmann, and Revelle examine the needs of particular fields of science -- biology, medicine, the behavioral sciences, and the earth sciences.[50] Levels of support, manpower requirements, and future needs are discussed in some detail. Three other papers, by Kantrowitz, Teller, and Willard, are concerned primarily with the relation between education and research. Kantrowitz focuses on problems of strengthening the applied physical sciences, and proposes a plan whereby qualified government and industrial laboratories would share in the educational function by supervising thesis work by graduate students on the premises.[51] Teller is similarly concerned with the inadequate emphasis given to applied science in graduate school curricula. He believes that the United States has slipped badly in the applied science fields, and endorses the suggestion for closer cooperation between government laboratories and universities to strengthen graduate education in the applied sciences. If necessary, legislation should be enacted to permit government-financed laboratories to spend a small fraction of their budgets for educational purposes where appropriate cooperative arrangements can be worked out with a university. Willard discusses the need for improved science education at the secondary level, and sees graduate education in the basic sciences as requiring a further infusion of federal research funds to support many worthy projects not now receiving support.

Bode's paper is a wide-ranging scholarly review of the changing relationship between science and technology from the early period of mutual isolation to the present growing interdependence.[52] He ends by stressing, with Kantrowitz and Teller, the need to strengthen the tie between basic and applied science. The paper is intended principally as background analysis, and as such contains few explicit policy recommendations. But the main thrust of Bode's analysis is clear: the United States needs to maintain a position of leadership both in science and in technology. The nation can no more have a flourishing science without technology than it can have a flourishing technology without science. Successful applications of basic discoveries are more difficult and demanding than is commonly appreciated (and are growing steadily more difficult). Consequently, there can be no slacking off in the efforts devoted toward achieving leadership in the technological field.

The remaining papers deal with a cluster of important tactical questions arising in connection with the support of science. The papers generally accept the premise that support for "science as a whole" is not a very useful concept as a focus for budgetary decisions. Much of society's support for basic science should remain with the mission-oriented agencies which provide support for those areas of science having a potential bearing on the agency's practical mission. Weinberg seems most insistent on the need to separate mission-oriented basic research from the basic science budget proper. He elaborates on the ideas developed earlier in his Minerva article, and seems to draw the distinction between the two categories of basic science support in sharper terms than he had previously. Other panelists follow his general line of reasoning, but see some drawbacks in this approach. Brooks and Verhoogen, in particular, point out that difficulties may arise if science budgets become overly sensitive to the normal fluctuations in agency budgets or to fluctuating opinions within an agency as to the relevance of particular basic research to the agency's mission. This leads a number of the panelists to a consideration of the National Science Foundation's crucial role as "balance wheel" in providing support to areas inadequately funded by the mission agencies. The National Science Foundation role during the next decade is seen, especially in the papers of Weinberg and Kaysen (as well as Brooks and Kistiakowsky), as perhaps the major concern of U.S. science policy. There is a strong suggestion that the National Science Foundation role during the next decade should become much greater than it has been in the past. The need for an expanded NSF budget will become particularly acute as the science budgets of mission agencies like the Atomic Energy Commission, the Department of Defense, and the National Aeronautics and Space Administration tend to level off and the fields which they have supported (especially the physical sciences) are caught in a squeeze.

The need for distinguishing between support for "big science" and for "little science" in basic research is another matter that concerns many of the panelists. There is some apprehension, spelled out most forcefully in the papers by Brooks, Kaysen, and Kistiakowsky, that a substantial part of what is considered support for basic science goes to "big science," i.e., research that is very expensive because of high equipment costs. This may give a distorted impression of the amount of support actually going to "little science," i.e., support for the individual investigator not involving large capital costs or what roughly can be considered the costs of academic basic science proper (but only roughly since some academic basic research is "big science" and some "little science" is done at other than academic institutions). The problem is that a great deal of money may be spent on "big science" which swells the budget for basic science and makes it appear that basic science is being handsomely supported, but in reality "little science" may be relatively neglected. As Brooks puts it, "The situation would be a little like building a new department store that was so expensive to keep open that it was necessary to fire all the salesmen...much of the planning for new research facilities that took place in fiscal years 1962 and 1963 was based on an implicit assumption of continuing expansion of research budgets. Now, in fiscal years 1964 and 1965, when these facilities are just coming into operation, the expenses of merely making them available -- without any science -- are confronting fixed or even declining budgets for basic research. The political embarrassment that would attend not using a facility already built makes it inevitable that the facilities are made available anyway, usually at the expense of the individual scientist who does not have large fixed costs. A recent calculation indicates that if the budget for oceanography continues to stay level, the cost of operating ships already planned but not yet completed will eventually consume almost the entire research budget."[53] In Brooks' view, and he is seconded by Kaysen and Kistiakowsky, special efforts are needed to assure adequate support for scientists engaged in "little science."

These panelists generally agree that different standards of judgment should apply to decisions on "big science" and "little science." Decisions on allocation of effort within "big science" must be more centralized and reflect more extensive participation of high-level government officials, because these decisions represent the largest expenditures and are basic investment decisions of the federal government. (It is clearly impossible, for example, to give every high-energy physicist his own accelerator.) With respect to "little science," however, there should be greater scope for the workings of the scientific market place and a more decentralized style of decision-making. In principle, every truly competent "little scientist" should be guaranteed support. Kistiakowsky estimates that the total costs for this category of support would be about $600 million a year at present, and should be expected to about double over the next five years. Definitions of "competence" (and thus estimates of the numbers of scientists who merit support as individuals) vary somewhat. Brooks seems most insistent on a strict definition of high quality, suggesting that perhaps only 5 per cent of all the scientists presently engaged in basic research merit support as individuals because of the intrinsic promise of their work

In view of rising costs for both "big" and "little" science, and in view of increased graduate student enrollments in the sciences, all of the panelists who explicitly discuss the question urge that basic science budgets will have to be gradually expanded over the next decade. Increased support appears necessary just to keep intact the present system of encouraging basic science.

Something in the order of a 15 per cent annual increase is suggested. A 10 to 15 per cent figure can also serve, in the opinion of several panelists, as a convenient rule-of-thumb indicator of an appropriate basic science "overhead charge" on the entire technological enterprise.

Another interesting perspective that emerges in several papers is the importance of "institutional choice" in questions of support for basic science. This focus appears to add something useful to the discussion. For, as Kistiakowsky insists, many important questions of scientific choice ultimately boil down to questions of institutional choice. Kistiakowsky and several other academic panelists argue that general, non-mission-oriented, basic research in the universities, above all, needs strengthening even at the expense of such basic research done in other institutions. Their argument is, first, that expanding educational needs give the universities first claim on federal support and, second, that the universities provide the climate most conducive to distinguished achievement in basic research. Kaysen sharpens the argument by adding the notion that support to the institutions most directly concerned with generating new knowledge and producing new scientists can provide something of a "reserve" of scientific capability which can be drawn on for various national needs when the occasion demands.[54] None of the panelists would like to see mission-oriented basic research taken away from government and industrial laboratories, and most believe that government and industrial institutions should do some general, non-mission-oriented basic research. The point at issue is where one draws the line between mission-oriented and non-mission-oriented basic research. Kistiakowsky and Brooks would perhaps draw the line somewhat more sharply than is done at present; their underlying fear seems to be that support to general basic research at non-academic institutions may present a squeeze on academic basic research in a period when basic science budgets are no longer expanding rapidly. Beyond this, however, greater attention to institutional choice considerations might have the healthy effect of partially correcting the present geographic imbalances in the distribution of scientific talent. It seems generally agreed that the project system of granting federal support to science may have tended to slow down the process of dispersion of scientific resources, which might not have been slowed down under different systems of support. Brooks recommends a gradual transition to a situation in which about 25 per cent of the costs of "little science" goes to supporting individual scientists, 25 per cent is institutional support, and about 50 per cent is project support.

Kaysen appears to go farthest in the direction of stressing the means of reaching decisions on science policy. If we can devise the appropriate administrative machinery, which will provide both deliberative and in some sense representative choices, then many of the substantive issues of science policy will be resolved as a matter of course. In this connection, Kaysen proposes an ingenious "tax" on "little science" to finance a part of the costs of "big science." "This cost-sharing arrangement," he suggests, "would appear as another useful administrative control device, directed toward making those representatives of any (scientific) field not themselves too directly concerned with using large facilities sensitive to their costs in terms of their own interests."[55] He sees some arguments against the "tax" device but, in the net, believes it would have the important advantage of furthering "the corporate sense of responsibility that continuing advisory committees tend to develop, even though their membership changes."[56] Another part of his proposal includes machinery for reaching deliberative choices on allocations between scientific fields. In general, Kaysen sees the search for appropriate administrative mechanisms as a more promising approach to the allocation of scientific resources than the inventing of new allocation formulae.

Most provocatively out of step with his colleagues is Harry G. Johnson, Chicago economist. Objecting that much of the contemporary argument for government support of basic scientific research puts it in the class of economically functionless activity, Johnson urges that "the argument that individuals with a talent for such research should be supported by society...differs little from arguments formerly advanced in support of the rights of the owners of landed property to a leisured existence.... Again, insistence on the obligation of society to support the pursuit of scientific knowledge for its own sake differs little from the historically earlier insistence on the obligation of society to support the pursuit of religious truth, an obligation recompensed by a similarly unspecified and problematical payoff in the distant future."[57] For a case to be made for government support, he insists that it must be shown that basic research yields a social return over its costs that exceeds the return on alternative investments of society's resources. His principal points of contention with his colleagues seem to be two: first, he wonders whether the assumption of a fixed resource (limited supply of scientific brainpower) is valid or whether the availability of scientific talent can be made responsive to market demands;[58] and, second, he asks whether non-government sources of support can meet a larger part of science's needs. Johnson agrees with Kaysen, however, that the market may not provide enough or the right balance of support.[59] Thus some government support will be necessary. But, he insists, the question remains whether the present commitment is too much or too little, and on this question we need much further thought and analysis before we can be confident of having any accurate policy guidelines.

The Academy report, I believe, has taken a large step toward filling in the outlines of Toulmin's call for "a clearer and crisper vision" of the questions at issue in a science policy. With further careful analysis of well-defined problem areas, science policymakers should have a much better idea of how to proceed in their complex task. It may be worthwhile, however, to call attention to the possible dangers of an overconcern with analytical rigor. For reasons already suggested, it appears self-defeating to "suboptimize" to the point of considering only very narrow and manageable questions. The larger questions cannot be left strictly to "value" or "political" judgment or the whole debate becomes pointless. Seen in this light one suspects that the Academy panel may have dismissed too readily the concept of "science as a whole" as a meaningful focal point of discussion. Questions of "balance" among fields of science, though elusive, are still a legitimate concern of policymakers and deserve some serious, even if inconclusive, reflection. Some thought to "science as a whole" will be difficult to avoid if, as is generally considered desirable, the National Science Foundation should occupy a more prominent role as "balance wheel" in supporting science during the next decade.[60]

VII

The Academy report, as the panelists recognize, still leaves a wide terrain for further inquiry. A few areas that seem particularly in need of further reflection and analysis deserve brief mention. It would seem important, as a follow-up to the Academy report, to have further efforts undertaken to specify the needs of particular science fields, and especially to identify neglected fields that may merit increased support. We need further analysis on the implications of regional imbalances in the distribution of scientific talent,[61] detailed studies of the actual costs and trends in support for basic science (especially as between capital costs, operating expenses, and what could be called actual research support),[62] consideration of the feasibility of greater use of cooperative international ventures to finance expensive lines of inquiry, and further exploration of the needs of "little science" as against "big science" and of the complex interplay between the two (including a detailed examination of how Kaysen's "tax" device might work). We also need to know a great deal more about future trends in the supply of and demand for scientific manpower of various kinds, the effect on scientific manpower utilization of massive technological programs, the usefulness and wisdom of international migration of scientific talent (the "brain drain" among industrial nations, the "neocolonialism" of attracting science students from the developing nations to the centers of advanced scientific research),[63] and the extent to which (if at all) international trade notions of "comparative advantage" may have relevance for a nation's scientific and technological activities.

Implicit in this "menu" of research items is the assumption that we need more and better statistical data on scientific and technical manpower and the whole spectrum of R&D activities. Research and analysis will be greatly hampered unless we develop a better data base to work with. As Brooks notes, "A recent report of the Organization of Economic Cooperation and Development has remarked that most countries have better statistics on poultry production than they do on the activities of their scientists and engineers."[64] The difficulties involved in improving such statistics will be formidable, but with sharper categories and better conceptualizations of what kinds of data are theoretically significant, important progress can be anticipated in the near future.

Also implicit in this list is the assumption that we need a better understanding of the complex interrelationship between government and science. Some feeling persists in the scientific community that science has become tainted and corrupted by the federal government's role as benevolent patron. This feeling probably can be in part written off as a nostalgic yearning for the austere purity of a bygone era.[65] Yet there are grounds for concern. The massive influx of federal research funds into the universities clearly has posed some difficult adjustment problems, and raised subtle questions about supporting science in ways that are consistent with the integrity of the scientific enterprise.[66] Some fear that the university "climate" is no longer conducive to creative work in pure science, with the distractions, administrative detail, and possibly subtle incentives to neglect good teaching associated with the quest for research funds.[67] A general concern is frequently voiced that Congress and the public are being led to think of science in terms of spectacular results like a moon landing or cancer cure. Consequently, there is an understandable anxiety that major imbalances in our scientific effort may result and that the massive ventures may drain off a disproportionately large share of the nation's scientific and technological talent.[68] What is clear is that we are not very clear about these questions; we need to know a great deal more about the government's impact on the universities, industry, and other institutions engaged in research activities.

In this connection it might be useful to take a close look at the "indirect costs" tangle.[69] Are there subtle incentives secreted in the details of administrative contracts which motivate professors to neglect teaching?

What basis is there to university complaints that inadequate indirect cost allowances distort the character and administration of American universities? Another research focus might be an examination of the role of the university-affiliated research institution, assessing the arguments that this sort of arrangement provides a convenient institutional "buffer" between the university and the government. An alternative approach might be to take a particular subject matter area -- say, oceanography -- and see what has happened as it passed from a period of relative undernourishment to one of greatly increased public interest and support. What have been the costs to pay, if any, in the sense of distractions, pressure for spectacular results, and so forth?

In the context of institutional choice, it might be worthwhile to attempt to gain a clearer idea of what portion of the national effort has gone (should go) to the *reproduction* of scientific knowledge as against its *exploitation* for current and future needs. This distinction, I think, may amount to something more than a familiar restatement of the importance of basic research. Consideration of a whole nexus of issues would be involved here -- the purposes of different institutional structures, professional incentive systems, career patterns, teaching practices, and science's contacts with the larger public. The aim would be to gain a clearer understanding of how policies for the support and for the use of science interact, reinforce each other, or conflict -- and, in the latter case, where the critical points of decision are. Perhaps we may be led to think less in terms of a rigid distinction between the basic science career and the applied science career. Other notions that have some intuitive appeal could be explored, such as the possibility of greater inter-institutional mobility for scientists, reorientation of graduate curricula to include more attention to applied science, and a more respectable role for the science writers who attempt to convey to the broader public an appreciation of the excitement of intellectual achievement. These sorts of problems may be particularly important for the developing countries who face understandable pressures for early results but who must nevertheless develop indigenous on-going scientific enterprises.[70]

Some final remarks seem in order concerning the notion of "planning" as it applies to science policy. There are at least two salient aspects that must be identified: first, "planning" in the gross sense that the national resources devoted to science should not be determined solely by the workings of the private market; and, second, "planning" in the more specific sense of influencing the actual distribution of scientific effort. The first is relatively non-controversial. It seems generally agreed that the rewards from many scientific and technological activities are too distant and uncertain to generate private support at anything like a socially optimum level. Hence, some government support, especially to basic science, appears inescapable. There remain some troubling questions of how much is enough and to what extent government support really leads to increased science rather than just bidding up the salaries and perquisites of those engaged in science. But on the whole there seems to be a fairly common basis for discussion.

The second aspect, however, is much more difficult. It is harder to define what "planning" might mean at this level, and opinions vary all the way from Polanyi's uncompromising guild syndicalism to those who would seek to manage science as part of a planned economy. No pretense is made that the many practical and conceptual difficulties involved here can be easily resolved. But a few tentative observations may be ventured. With respect to the actual distribution of scientific effort it seems highly desirable to avoid an overcentralization in the style of decision-making. Science and technology are activities characterized by large uncertainties, and numerous decisions at various levels within a variety of public and private organizations do and should influence the distribution of scientific effort. Thus it is doubtless illusory to think in terms of a central Department of Science entrusted with some broad mission of coordinating all support for science. The present loose-jointed system of support, reflecting the participation of numerous government agencies and their congressional allies and critics, offers a better assurance that science in general will be strengthened than would any single focal point of decision. Yet one is left with the impression that imbalances in the distribution of effort and misallocations of resources have resulted from the present system. Similarly, the arguments for the self-equilibrating nature of scientific growth do not seem entirely convincing. Some measure of "planning" may be necessary to rationalize the present pattern of science support (if only to plan for the more efficient working of the scientific "marketplace").

Fortunately, in recent years the notion of planning has been discussed on a higher level of sophistication than used to be the case. Debates about planning have left the realm of grand alternatives and have revolved more around questions of particular techniques, institutional arrangements, and subtle adjustments in incentive systems.[1] The concept of the plan may allow for numerous areas where private consumer -- or "unplanned" -- behavior prevails. The assumption here is a similar one: planning for the allocation of effort within science should represent an interplay of public and private initiatives in an essentially pluralist framework with scope for both government influence and market mechanisms. This would imply a greater effort by the federal government to articulate consistent goals in science policy, to correct misallocations of effort resulting from defects of the scientific "market," and to understand how government actions influence the distribution of scientific effort and to exercise that influence in a more systematic and predictable fashion. It seems clear at least that more long-range thinking, analysis of trends, and critical evaluation of current practices are called for if we are to develop wise strategies for the encouragement of science.

FOOTNOTES

1. Derek J. De Solla Price, Little Science, Big Science, Columbia University Press, New York, 1963.

2. As Stephen Toulmin observes, "...once calls for expenditure on research and development exceed some 1-1/2 to 2-1/2 per cent of the gross national product, the sums involved inevitably--and properly--become subject to political scrutiny alongside other comparable items in the national budget, such as social insurance, schools and defence." "The Complexity of Scientific Choice: A Stocktaking," Minerva, II (Spring 1964) 3, p. 343. On the relation of science and government, the reader is especially referred to Don K. Price, "The Scientific Establishment," Proceedings of the American Philosophical Society, Vol. 106, No. 3, June 1962, and Government and Science, New York University Press, New York, 1954.

3. "Criteria for Scientific Choice," Minerva, I (Winter 1963) 2, and "Criteria for Scientific Choice II: The Two Cultures," Minerva, III (Autumn 1964) 1.

4. "Criteria for Scientific Choice," p. 161.

5. Ibid., p. 159.

6. "Criteria for Scientific Choice II: The Two Cultures," p. 5.

7. Ibid., p. 12.

8. "The Republic of Science: Its Political and Economic Theory," Minerva, I (Autumn 1962) 1.

9. Ibid., p. 62.

10. Minerva, II (Winter 1964) 2.

11. Ibid., pp. 145ff.

12. Ibid., p. 155.

13. Minerva, I (Winter 1963) 2.

14. Ibid., p. 179.

15. Ibid., p. 175.

16. An effort to assess some of these elusive factors is Vernon Van Dyke, Pride and Power: The Rationale Behind The Space Program, University of Illinois Press, Urbana, 1964.

17. For a discussion of this point, see the essays by Harvey Brooks and John Verhoogen in Basic Research and National Goals, National Academy of Sciences, Report to the Committee on Science and Astronautics, U. S. House of Representatives, March 1965.

18. As Don K. Price cautions, "We probably have less reason to fear that major government decisions involving science will be secret than that they will be popular." "The Scientific Establishment," p. 244. Price elaborates on the dangers of popular misunderstandings of science, and especially the tendency to consider science a mere supporting arm of technology, in "The Established Dissenters," *Daedalus*, Winter 1965, pp. 84-116.

19. Maddox, "Choice and the Scientific Community," p. 142.

20. *Ibid.*, p. 155.

21. Derek J. De Solla Price, *Science Since Babylon*, Yale University Press, New Haven, 1961, Ch. 5, "Diseases of Science," pp. 92-124; Toulmin, "The Complexity of Scientific Choice: A Stocktaking," pp. 351-352; Bernard Barber, "Resistance by Scientists to Scientific Discovery," in Bernard Barber and Walter Hirsch, *The Sociology of Science*, The Free Press, Glencoe, 1962, pp. 539-556.

22. Wallace S. Sayre, "Scientists and American Science Policy," in Robert Gilpin and Christopher Wright, eds., *Scientists and National Policy-Making*, Columbia University Press, New York, 1964, pp. 144-173.

23. *Minerva*, II (Spring 1964) 3.

24. *Ibid.*, p. 349.

25. *Ibid.*

26. *Ibid.*, pp. 343-344.

27. *Ibid.*, p. 354.

28. *Ibid.*

29. *Ibid.*, pp. 354-355.

30. See the treatment of the "ecology" of government in John M. Gaus, *Reflections on Public Administration*, University of Alabama Press, University, 1947.

31. Toulmin, "The Complexity of Scientific Choice: A Stocktaking," p. 354.

32. *Ibid.*, p. 355.

33. For a critique of this thinking, see Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age*, Harvard University Press, Cambridge, 1960, and James R. Schlesinger, "Quantitative Analysis and National Security," *World Politics*, Vol. 15, No. 2, January 1963.

34. Toulmin, "The Complexity of Scientific Choice: A Stocktaking," p. 357.

35. *Science and the Policies of Governments*, Report of the Organization for Economic Cooperation and Development, Paris, September 1963, and Harvey Brooks, "The Scientific Adviser," in Gilpin and Wright, eds., *Scientists and National Policy-Making*, pp. 73-96.

36. "The Scientific Adviser," pp. 76-77.

37. See the various studies by Gaus and Don K. Price cited above and James L. McCamy, *Science and Public Administration*, University of Alabama Press, University, 1960. McCamy sees the "social fallout" from science and technology as a major preoccupation of public administration in the future.

38. Robert Gilpin, *American Scientists and Nuclear Weapons Policy*, Princeton University Press, Princeton, 1962; Warner R. Schilling, Paul Y. Hammond, and Glenn Snyder, *Strategy, Politics, and Defense Budgets*, Columbia University Press, New York, 1962; Schilling, "The H-Bomb Decision: How to Decide Without Actually Choosing," *Political Science Quarterly*, LXXVI, No. 1, March 1961; Bruce L. R. Smith, "Strategic Expertise and National Security Policy: A Case Study," *Public Policy*, XIII (Yearbook of the Graduate School of Public Administration of Harvard University), 1964; some useful discussion has also emerged in the polemics surrounding C. P. Snow's *Science and Government*, Harvard University Press, Cambridge, 1961. See esp. Albert Wohlstetter, "Strategy and the Natural Scientists," in Gilpin and Wright, eds., *Scientists and National Policy-Making*, pp. 174-239.

39. Robert C. Wood, "Scientists and Politics: The Rise of an Apolitical Elite," in Gilpin and Wright, *Scientists and National Policy-Making*, pp. 41-72.

40. Price, Wohlstetter, and Sayre, in studies cited, may be said to incline toward this view.

41. Warner R. Schilling, "Scientists, Foreign Policy, and Politics," in Gilpin and Wright, *Scientists and National Policy-Making*, pp. 144-173.

42. Brooks, "The Scientific Adviser"; Robert N. Kriedler, "The President's Science Advisers and National Science Policy," in Gilpin and Wright, eds., *Scientists and National Policy-Making*, pp. 113-143; D. Allison, "The Science Brain Trust," *International Science and Technology*, January 1965, pp. 61-68; Ernest A. Engelbert, "The Representativeness of Selected Federal Science Advisory Bodies," unpublished manuscript, University of California, Los Angeles, 1965; C. W. Fisher, unpublished manuscript, Harvard, 1964.

43. There has been a growing concern in Congress with the question of the representativeness of federal science advisory bodies. Recent appointments to the President's Science Advisory Committee seem to reflect this concern as the Harvard-MIT "presence" has declined somewhat. For a brief commentary, see D. S. Greenberg, "Advisory Set: New Appointments Reduce Harvard-MIT Presence on President's Science Committee," *Science*, April 16, 1965, p. 352.

44. *Reorganization Plan No. 2 of 1962*, U. S. House of Representatives, H. R. Doc. No. 372, 87th Congress, 2nd Session, Washington, 1962.

45. See, for example, Clinton P. Anderson, "Scientific Advice for Congress," Science, April 3, 1964.

46. Traditionally, the National Academy of Sciences has worked with the executive agencies and remained generally aloof from the Congress. In 1964, however, the Academy entered into a formal arrangement with the House Committee on Science and Astronautics to provide advice on science policy. The Academy report, Basic Research and National Goals, was the first assignment done for the House Committee. In the view of Committee Chairman George P. Miller (D-Cal.), this report "represents not only genuine achievement and utility in itself, but a significant milestone in congressional methods of gathering talented, objective assistance to its use." See below, pp. 23-33.

47. In this connection, one line of Wildavsky's defense of the rationality of the present budgetary system is instructive: Congress has enough information to exercise a useful review function with respect to the executive budget but not so much information that it is tempted to redo the work of the executive agencies. See Aaron Wildavsky, The Politics of the Budgetary Process, Little, Brown and Company, Boston, 1964.

48. Basic Research and National Goals, National Academy of Sciences, Report to the Committee on Science and Astronautics, U. S. House of Representatives, March 1965.

49. The panelists were, besides Kistiakowsky, Lawrence R. Blinks, Stanford University; H. W. Bode, Bell Telephone Laboratories; Harvey Brooks, Harvard University; Frank L. Horsfall, Sloan-Kettering Institute for Cancer Research; Harry G. Johnson, University of Chicago; Arthur Kantrowitz, Avco-Everett Research Laboratory; Carl Kaysen, Harvard University; Saunders MacLane, University of Chicago; Carl Pfaffmann, Brown University; Roger Revelle, Harvard University; Edward Teller, University of California, Berkeley; John Verhoogen, University of California, Berkeley; Alvin M. Weinberg, Oak Ridge National Laboratory; and John E. Willard, University of Wisconsin.

50. Blinks, "Opportunities and Requirements in the Life Sciences," pp. 25-40; Horsfall, "Federal Support of Biomedical Sciences," pp. 111-126; Pfaffmann, "Behavioral Sciences," pp. 203-236; and Revelle, "The Earth Sciences and the Federal Government," pp. 237-256.

51. "Leadership in Applied Physical Science," pp. 143-146. Weinberg had proposed a similar plan in 1962, and recently the Oak Ridge National Laboratory (of which he is the Director) entered into a cooperative arrangement with the University of Tennessee to implement some of the suggestions. See John Finney, "U.S. Will Examine Its Laboratories," New York Times, September 6, 1964, p. 37.

52. "Reflections on the Relation between Science and Technology," pp. 41-76.

53. Harvey Brooks, "Future Needs for the Support of Basic Research," pp. 80-81.

54. Carl Kaysen, "Federal Support of Basic Research," pp. 147-168, esp. 150-151. Historical evidence in support of this notion is adduced in Joseph Ben-David, "Scientific Productivity and Academic Organization in Nineteenth-Century Medicine," in Barber and Hirsch, eds., The Sociology of Science, pp. 305-328. Ben-David argues that German academic organization in the nineteenth century provided a reserve of scientific capability which enabled that nation to capitalize on recent discoveries and to achieve primacy in the field of medical research at the turn of the century.

55. Kaysen, "Federal Support of Basic Research," p. 161.

56. Ibid.

57. Johnson, "Federal Support of Basic Research: Some Economic Issues," p. 132.

58. A similar assumption was made earlier in A. A. Alchian, K. J. Arrow, and W. M. Capron, An Economic Analysis of the Market for Scientists and Engineers, The RAND Corporation, RM-2190, 1958. Cf., however, Vladimir Stoikow, "The Allocation of Scientific Effort: Some Important Aspects," Quarterly Journal of Economics, Vol. 78, May 1964, pp. 307-323.

59. For a further analysis of this point, see Richard R. Nelson, "The Simple Economics of Basic Scientific Research," Journal of Political Economy, Vol. LXVII, No. 3, June 1959, pp. 297-309.

60. A reflection of the increased interest in NSF's role is the recently announced congressional investigation of NSF operations. This will be the first comprehensive congressional review of that agency's functions since its inception in 1950. See "News in Brief: Hearings on NSF Announced," Science, May 7, 1965, p. 775.

61. See Federal Support of Basic Research in Institutions of Higher Learning, Report of the Committee on Science and Public Policy, National Academy of Sciences, Washington, 1964. Congress has become increasingly concerned with this issue, and has recently taken a step that could signify an important shift toward a more active congressional role in encouraging geographic dispersion of scientific resources. For the first time, a congressional subcommittee wrote into an appropriations bill a limitation on the percentage of NSF fellowships that can be awarded to any one state. If this limitation survives in the final version of the bill, Congress will have indicated a clear intention that executive agencies take the geographic dispersion issue more seriously than they have in the past. See Independent Offices Appropriations for 1966, Hearings before the Independent Offices Appropriations Subcommittee of the Committee on Appropriations, House of Representatives, part 2, Washington, 1965. For a commentary, see Daniel S. Greenberg, "NSF Budget: Cuts by House Group Leave Little Leeway for Growth in Support of Research Projects," Science, May 14, 1965, pp. 928-930.

62. See Harvey Brooks, "Future Needs for the Support of Basic Research," pp. 77-84, for a discussion of the inadequacies of present R and D statistics. One of the

few studies available on this subject is E. D. Brunner, *The Cost of Basic Research Effort: Air Force Experience, 1954-1964*, The RAND Corporation, RM-4250, February 1965. On the Russian experience, see Alexander Korol, *Soviet Research and Development: Its Organization, Personnel, and Funds*, MIT Press, Cambridge, 1965, and Nancy Nimitz, *Soviet Expenditures on Scientific Research*, The RAND Corporation, RM-3384, January 1963. A substantial comparative study, *The World of R and D*, by Arnold Kramish of The RAND Corporation is in progress.

63. Stevan Dedijer, "Why Did Daedalus Leave?" *Science*, June 30, 1961, pp. 2047-2052.

64. "Future Needs for the Support of Basic Research," pp. 77-78.

65. As Don K. Price comments, "One may well be a little skeptical about this point of view, and suspect that poverty probably brought its distractions no less troublesome than those of riches." "The Scientific Establishment," in Gilpin and Wright, eds., *Scientists and National Policy-Making*, p. 39.

66. See, *inter alia*, Harold Orlans, *The Effects of Federal Programs on Higher Education*, The Brookings Institution, Washington, 1962; Charles V. Kidd, *American Universities and Federal Research*, Harvard University Press, Cambridge, 1959; Douglas M. Knight, ed., *The Federal Government and Higher Education*, American Assembly, Prentice-Hall, Inc., Englewood Cliffs, N. J., 1960.

67. See, for example, the remarks of Frederick Seitz, President of the National Academy of Sciences, at a Purdue University symposium, as reported by Walter Sullivan, "New Plan to Aid Science is Urged," *New York Times*, April 15, 1965, p. 38 and Intriligator and Smith papers cited in bibliography.

68. See *Toward a Better Utilization of Scientific and Engineering Talent: A Program for Action*, A Report of the Committee on Utilization of Scientific and Engineering Manpower, National Academy of Sciences, Publication 1191, Washington, 1964.

69. *Indirect Costs Under Federal Research Grants*, Government and Science No. 5, Report of the Committee on Science and Astronautics, U. S. House of Representatives, 89th Congress, 1st Session, Washington, 1965, and the studies cited in note 66.

70. Ruth Gruber, ed., *Science and the New Nations*, Basic Books, Inc., New York, 1961.

71. Robert A. Dahl and Charles E. Lindblom, *Politics, Economics, and Welfare*, Harper & Brothers, New York, 1953; P. J. D. Wiles, *The Political Economy of Communism*, Harvard University Press, Cambridge, 1962; John and Anne-Marie Hackett, *Economic Planning in France*, Harvard University Press, Cambridge, 1963.

APPENDIX II

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"The Economy of Science: The Proper Role of Government in the Growth of Science"

MINERVA, SPRING 1981

The Economy of Science: The Proper Role of Government in the Growth of Science

Simon Rottenberg

THE progress of the American economy and the improvement of the conditions of life of the American people in the present century have depended in part on the achievement of scientists and scientifically trained technologists and on the introduction of the results of their work into industrial and commercial practice.[1] Governmental policy in the United States in the second half of this period has had a very large influence on the scale, the composition, and the quality of the country's scientific and technological achievement.

Scientific research produces knowledge of nature, life and society. Each generation inherits a stock of scientific knowledge which it increases and improves and which it then transmits to succeeding generations. Each generation which engages successfully in scientific research adds to the intellectual capital of society.

Research is conventionally divided into basic research, which seeks more fundamental knowledge of a phenomenon but not its practical application, and applied research, which is directed toward making usable discoveries towards the practical application of already acquired knowledge. Technology, which is sometimes called development, is the use of scientific knowledge in order to produce useful materials, devices, systems or methods. Technology includes the design and improvement of prototypes and processes.[2]

Some of the commodities and services produced by technology contribute in their turn to the production of scientific knowledge. There is a reciprocal relationship between science and technology.

Scientific and technological research employs resources which have alternative uses each of which has value for society. Those resources are scarce and their use in research imposes upon society what the economists call an "opportunity cost" by pre-empting their use from other purposes. Resources that are put to any particular use in scientific research also impose opportunity costs because they are not then available for the conduct of other scientific investigations.

Research requires the employment of resources when the research is undertaken, but, if the research is successful, there is a lag between the time when the research was begun and carried on and the time when successful discovery occurs. Some research projects are close to, and others are distant from, the threshold of discovery. Furthermore, some increments of knowledge acquired by applied and technological research will yield commodities and make possible services which will perhaps be used by society for a long time after the research is successfully completed. Thus the costs of research—in the form of the forestalled alternative uses of the research resources—are incurred early but the stream of benefits that the research yield to society is enjoyed later. The present is preferred to the future; we observe that, in many situations, human beings are paid so that they will consent to postpone consumption from which they derive utility and that they pay to bring consumption forwards in time. They must be paid more if the postponement is for a long time than if it is for a short time. The more distant from the present is the period in the future in which the benefits of an activity are to be enjoyed by society, the less is its value in the present. Future streams of service must be discounted at some rate to determine their present value. The proper rate at which future benefits should be discounted is one that expresses the disutility of postponement, and a proxy for it can be found in the interest rates that are currently paid, adjusted for inflation. Applied scientific research which is undertaken in the present and which will yield its benefits to society only in the very far distant future probably wastes society's resources and should not be done. There are better uses to which society can put the resources that would be employed in that research. Activities which require that costs be incurred early and which yield their products later are called investments. Expenditures on scientific and technological research, since they have those properties, are, therefore, called investments.

Research is a risk. Some research projects are successful and others are failures. The distribution of the outcomes of research is a continuum; there are various degrees of success and various degrees of failure. Society is composed of individuals some of whom prefer to take risks, others who are averse to risk, and still others who are neutral in this regard. Whether a risky activity will be undertaken depends upon an estimate of the probability of a particular outcome, and upon the intensity of aversion to risk in the society.

As in any other venture that seeks to produce something, there are, in research, alternative processes of production which can be employed to secure that outcome. Production of a scientific outcome requires the combination of factors of production and the choice of a productive technology among the alternatives which are available. The proportions in which factors of production are combines are not fixed. Factors are substitutable, one for another, even if they are not perfect substitutes. Less of one can be compensated for by the employment of more of another.

These are the conventional considerations taken into account by economists when, applying a criterion of efficiency, they elaborate the principles of rational production and the choice of investment. These are the principles on the basis of which attempts are made to answer the questions: what to produce?, how much to produce?, how to produce?, and, for investments, when to produce?

Some Elementary Economic Considerations

The discipline of economics has constructed a set of formal conditions which, if fulfilled, will cause socially optimal outcomes of production and investment to occur. The conditions of optimality are derived from the applications of methods of logical analysis to a model which postulates that actors in the economy seek, subject to constraints, to maximise the realisation of their ends. To illustrate those conditions in simple form, they include prescriptions, like those which follow. The quantity of output to be produced in each activity should be such that the last unit of a product will have a value which is equal to the value of the resources employed to secure that increment of output. Resources of different kinds should be combined in production so that the contribution to output of the last units of each resource (in a quantitative, and not a temporal sense) will be proportional to their prices, and the last dollar's worth of each factor of production will make the same contribution to output. Fulfilling that condition permits output to be acquired at the lowest cost in resources. It also maximises output from the expenditure of given resources. For activities that employ resources currently but yield their products only later, the discounted value of the stream of net products over time should be at least equal to the value of the resources that are currently employed and the rate of return should be equal in all such activities.[3]

Economic theory asserts that competitive markets, in which there is freedom to enter and depart and in which choice among alternatives is made by households and firms will permit the conditions of optimality to be fulfilled and socially efficient outcomes to occur, except in two special cases.[4] The market is an efficient instrument for deciding the uses to which the resources of society should be put. The individuals who compose society differ greatly from one another, in the values they put upon commodities, services, risk, uncertainty, and postponement, and in the estimation of the future. They also differ because the variables which affect the valuation of things by individuals is very large. The market aggregates an immense quantity of information with the result that a set of social evaluations is constructed. The market puts value upon the social opportunity costs of resources, and produces the social valuation of benefits, social discount rates, a social expression of intensity of aversion to risk and social estimates of future occurrences. All of these are components of the processes by which decisions are made about the uses to which resources should be put in order to maximise the welfare of society. The aggregated values appear and are expressed in the market as prices of resources and products and as interest rates.

Decisions on the allocation of resources can be made, not only by the aggregation of the decisions of individuals, households, and firms through the market, but also by collective, political processes in which holders of public office, elective and appointive acting as surrogates of society, make decisions. In that case, either the preferences of officials are imposed upon society or they seek to estimate the sets of preference of the society and to make the allocative decisions that society would have made, if aggregated individual preferences guided the decisions. To do the latter requires the assimilation and correct treatment of enormous quantities of information; the bureaucratic replication of these preferences cannot be done in most cases. At the least, it will be done less efficiently than if markets were permitted to function. Resources will not be put to their most highly valued uses and the welfare of society will not be maximised, given the scarcity of resources.

In two cases, however, the market will fail, inefficiency will occur and resources will be put to the wrong uses, if markets are allowed to operate freely. These are the cases where the product of an activity is a "public good" and where there are "externalities", as when the private costs or benefits of an activity are different from its costs or benefits to society as a whole, The two cases are somewhat similar. Public goods are those for which it is not possible or it is administratively very difficult to exclude some persons from the benefits they offer. The classic example is the lighthouse. If a lighthouse is constructed to mark a shoal, all passing ships will receive the signal. Fees cannot be conveniently charged for the receipt of the signal, so that none can be excluded from receiving it.

No private shipowner, in deciding whether to construct a lighthouse,

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[1] There have been a small number of studies of the effects of research and development on economic growth and on the productivity of resources employed in production. See Terleckyj, Nestor E., "Estimates of the Direct and Indirect Effects of Industrial R & D on Economic Growth", in Terleckyj, N. E. (ed.), *The State of Science and Research: Some New Indicators*. (Boulder, Colorado: Westview Press, 1977), pp. 123-146. See also Denison, Edward R., *The Sources of Economic Growth in the United States and the Alternatives Before Us* (New York: Committee for Economic Development, 1952), pp. 229-255.

[2] The classification of scientific and technological effort into these categories is not done without difficulty and requires the exercise of judgment because the categorical definitions overlap. The published data on federal government expenditures for research vary among sources; depending upon whether the classification of expenditures into categories of basic research, applied research, and development is done by the institutions performing the research or by the governmental agencies supporting the research. These definitions are drawn from National Science Foundation, *Review of Data on Research and Development*, (April 1962), p. 8, quoted in Kendrick, John W., *The Formation and Stocks of Total Capital* (New York and London; Columbia University Press, 1976), p. 8.

[3] The conditions of optimality and the exposition of the logical processes by which they are produced can be found in more complete and sophisticated form in any conventional intermediate text book in intermediate microeconomic theory. See, for example, Mansfield, Edwin, *Microeconomics, Theory and Applications*, 3rd edn. (New York and London: W. W. Norton, 1979); Hirshleifer, Jack, *Price Theory and Applications*, 2nd edn. (Englewood Cliffs, N. J.: Prentice-Hall, 1980).

[4] Mansfield, E., *op. cit.*, p. 451.

will take account of the benefits it will yield to other shipowners whose ships are kept from being beached because the signal marks the shoal. A shipowner building a lighthouse will bear the whole cost of its construction but he will acquire only part of the gain. The market would cause too few lighthouses to be built.

Public goods also have the property that their consumption by some persons does not deprive others of consuming them. If one ship observes the lighthouse's signal, no less of the signalling capacity of the lighthouse will be available to other ships.

"Externalities" occur when costs fall upon, or benefits are enjoyed by, some who are not parties to the activity. The classic example is the steel mill billowing from its chimneys smoke and particulates that fall on neighbouring residences which are downwind from the mill. Sellers and buyers of steel take no account of those costs in making decisions that determine how much steel will be produced and how it will be produced. The market will cause too much steel to be made, from the point of view of the whole society, because producers of steel will ignore those costs of making steel that fall upon other persons.

In those cases, since the market fails and the processes of the market will cause either too little or too much of a product to be produced and too few or too many resources to be devoted to its production, the representatives of the society, acting through political institutions should intervene in order to bring about a socially appropriate allocation of resources.

The intervention of government, in these situations, can take such forms as the production of the relevant product by governmental agencies themselves, or by governmental subsidies or taxes which will alter the incentives and deterrents confronting private firms and thus cause them to alter their productive decisions.

There is yet another case in which products that have value for society will not be produced in sufficient quantity unless government acts. Those are products for which one governmental agency, or a very small number of governmental agencies, is the only buyer. Military submarines and armoured personnel carriers might be built by private companies and held in inventory awaiting the appearance of buyers, as private companies now produce and hold inventories of pencils and shoes. To be successful, the companies would have to forecast correctly future military procurement and the rates at which different branches of the military establishment will grow. The risk is too large; submarines and personnel carriers will not be built unless governments build them or contract to have them built.

All of this has implications for the allocation of resources to, and within, scientific and technological research.

The Allocation of Resources to Scientific Research

4

Research employs scarce resources. Its scale should be that which fulfils the condition of optimality that cost and output should be of equal value at the margin. Similarly, the scale of research in each field should fulfil that condition. This means that the scale of research activity and of any part of research activity can be too large, when the costs of resources exceed the value of their output at the margin, and it can be too small, when the value of output exceeds resource costs at the margin.

Research can be undertaken in different ways. The resources which are employed in research should be combined in the way that will cause the marginal contribution of resources to output to be proportional to their prices. This means that research activity can be undertaken in the wrong places and institutions, that it can be done by the wrong persons, and that its form of organisation can be wrong.

Research that is done currently yields its fruits only later. An "investment project" in research should be undertaken only if the discounted value of the stream of benefits it will produce exceeds the cost of resources put into the research and only if there are not other ventures available that will yield even higher rates of return on investment, or only if the rate of return on the research "investment project" is at least equal to the rate of interest. This means that wrong fields of research and wrong projects can be undertaken.

These are extraordinarily difficult conditions to fulfil in an explicit and rigorous manner. If they are not fulfilled, society wastes its resources by putting them to inappropriate uses. The market permits these conditions to be fulfilled, however, even though explicit calculation does not occur.

Science policy is a set of public policies which attempts to influence the rate of growth of the stock of knowledge, the fields within which the growth occurs, and the rate of the transformation of knowledge into the production of useful commodities and services.

Those who design science policy should recognise the limitations on the power of public officials correctly to calculate the socially optimal use of resources in research. The administrators of governmental science policy should be specialised in the support of ventures in research in which markets can be expected to fail. Where the market can operate efficiently to decide which research activities should be undertaken, what their scale should be, how they are to be carried out and in which institutions they should be done, the market should be permitted to do so without the intervention of government which would distort the outcome.

Market failure can be expected most clearly in the case of basic research and this is the field in which it is clearly right for government support to be engaged. Basic research produces increments of scientific knowledge with no intended practical purposes. Those who engage in this kind of research activity cannot exclude others from sharing the fruits of their labour. They cannot themselves acquire the whole value of their products. Indeed, it would not be socially useful to exclude others from sharing these products, even if it were possible to do so, for it is characteristic of basic scientific knowledge that its possession by any one does not prevent its possession by others. It is desirable to subsidise basic research. In the absence of subsidy, the output of basic scientific knowledge would be too small in the sense that additional quantities of it could be produced at incremental costs in resources that are less than the value of the increments of knowledge they would generate. Some resources used in other industries would have more productive uses in this one.

It is important to understand, in this regard, that scientific knowledge is, in itself, a useful product. It is a public consumption good. It is appropriate to use some of the community's resources to enlarge the stock of knowledge for its own sake. If, in addition, new knowledge generates new or better commodities and services or makes resources more productive in their transformation into products, so much the better. Basic research that yields pure knowledge is valuable for society.

Too much should not be made of the defence for an active and extensive public science policy in terms of service as a public good. Patentable discoveries should be excluded. For centuries, persons engaged in scientific research have made unpatentable or unpatented discoveries and released them to the community at large. Such persons are rewarded by their awareness of contributing to the growth of knowledge, by professional repute and the respect of their peers. Indeed, rewards of that sort cannot be procured unless discoveries are revealed.

One must be careful, in the application of the defence for science policy 5
in terms of service as a public good, that public goods are properly perceived. Clean air is a public good. Exclusion from its consumption is not possible; if one consumes a unit of it, it is available in such abundance, that others are not deprived of the quantities of it which they desire to have. Nevertheless, research to produce knowledge that will cause the air to be cleaner is not necessarily, as a consequence, also a public good. Suppose government sets a maximum standard for carbon monoxide emissions from motor-car exhausts. Manufacturers of motor-cars will seek out low-cost and efficient instruments for meeting the standard to install

in their products. A scientist seeking knowledge that will permit the design of such an instrument will be able to acquire the whole of the gain produced by the knowledge he discovers; he will sell the knowledge to motor-car manufacturers or to their suppliers. The market will not fail. The socially appropriate quantity of resources will be devoted to the search for the relevant increment of knowledge. That clean air is a public good does not cause research seeking knowledge of methods for making the air clean also to become a public good.

Historically, an extensive governmental activity directed toward the increase in scientific and technological knowledge has not been a necessary condition for scientific and technological progress. The growth of scientific knowledge occurred for many centuries before governments began deliberately to support research and development. Scientific and technological revolutions and a radical transformation of intellectual tradition have occurred without direct governmental subvention and certainly without public direction of scientific choice.

An active science policy which proceeds on the postulate of science as a public good in the case of basic research must also be employed with care because there are grey areas in which the practical consequences of basic research discoveries might be foreseen. It is not clear, in such cases, whether the research is clearly and fundamentally basic and hence warrants governmental support, rather than permitting processes of the market to decide whether it should be undertaken and how much of resources should be devoted to it. Examples can be drawn from the National Science Foundation's recently published report on the outlook for science and technology.[5]

It is reported there that "basic knowledge in such fields as chemistry, enzymology, and plant physiology is needed to understand the mechanisms by which plants and bacteria collect, store and convert or release the energy of the sun" and, if it were known the use of photosynthesis to generate convenient energy resources would be advanced.[6] If this is true, companies producing commodities that use sources of energy and their suppliers should then perhaps find it profitable to engage in the production of knowledge of this type which can be transformed into energy-producing capacity and from which they can acquire the monetary benefit of the research. If investment in the acquisition of the new basic knowledge were estimated by firms such as those to yield a return, in the form of gains from new energy-generating capacity, that would be smaller than the return yielded by other kinds of investment, they would not undertake the research. It would be wasteful for them to do so because, in those circumstances, the resources devoted to that research would be more productive if employed in other activities. They would be wasted if they were used in research. If private firms would not undertake the research in such cases, it would also be wasteful for government to support research of that kind. If governmental agencies financed that research, they would be wasting society's resources.

It is said that "better understanding of the chemistry of the nervous system can help with behavioral problems that lead to obesity, drug and alcohol abuse, and such disorders as schizophrenia, senility, and mental retardation".[7] Pharmaceutical manufacturing firms should find such basic research financially interesting.

Basic research on catalysis, it is said, will be of vital importance to the chemical and petrochemical industries.[8] Firms in such industries should find it financially rewarding to undertake such basic research. "Waste products from industrial physical, chemical, and biological processing consist of complex compounds and mixtures of substances. A better understanding of them is likely to lead to new concepts for recovery of their resource value."[9] Firms that produce waste in large quantity might gain by engaging in research on those substances.

"Basic research . . . on the production of potentially 'new' materials [has] potential applications [that] include amorphous . . . substitutes for low-loss transformer cores, amorphous semiconductors for use in electronic device applications, photosensitivity polymers . . . for dry lithography."[10] If these materials are economically useful, private firms and their suppliers might do such research; it would be to their financial advantage.

"Advances in plant tissues culture and recombinant DNA research provide the basis for a foreseeable new technology in which plants can be modified and desirable new traits introduced."[11] Firms producing plants, seeds and forest products should find such research of financial interest.

If the consequences of research are known and the financial gains from the research can be obtained by its discoverer, the knowledge that is discovered is not a public good, the knowledge is not pure, and subvention by the State is not clearly called for. If it is observed that commercial firms are not engaged in such research, it is possibly because the cost of discovery exceeds the gains to be yielded by it, or the risk and uncertainty of the research is too large, given the degree of intensity of aversion to risk that prevails in the society. If private firms, considering the demand for the substance which research might discover, decide that it will be too small to repay them for the expenditures required for the research, they will not undertake it. If, in the circumstances, governmental subvention none the less, causes the research to be undertaken, it is a proper inference that government is wasting resources. To know whether such research should be done, one must see whether the market causes it to be done. If the decisions made by the market do not do so, it should be concluded that the resources have higher productivity in other uses and should not be used for the research in question.

When it is appropriate to do so, firms in the private sector will engage in scientific research even if there is a risk that the research might fail to produce the knowledge that is sought. The process is analogous to the search for petroleum. Seismographic surveys and the examination of samples of surface rock and of core drawn from the sub-surface provide information about the geological sub-structure of areas of land-mass and disclose where petroleum might be located. It cannot be known with certainty, however, whether petroleum is present in any particular place until wells are drilled. In the course of American history, eight dry wells have been drilled for each well that has been successful in the exploratory search for petroleum. Wells will not be drilled at all, however, in places where the seismographic and geophysical work do not predict with a fairly high probability that petroleum might be located below the surface of the land. If drilling were done in such unpromising places the ratio of failures to successes would be much larger than eight to one. When the market determines the quantities and kinds of applied scientific research that will be undertaken by private firms, a socially appropriate quantity of risk tends to be taken. If the risk associated with a particular venture in applied scientific research is too large, the research will not be done; it should not be done, if the risk is large enough and society's resources are put to their proper uses.

Basic Research and Public Subsidy

Since the market fails in the case of research that is truly basic which produces public goods in the form of pure knowledge with no known or intended practical applications, it is the proper specialised province of government to invest in such research, if an optimal quantity of it is to be produced.

Public policy then confronts the problem of defining the scale of that investment. It is the same problem confronted by policy in settling on the number of lighthouses, levees, and monuments that should be built, the amount of land to be set aside as wilderness, and the quantity of river dredging that should be done. They are all public goods. The market fails in those cases as well and governmental subsidies are necessary, if the correct quantity of resources is to be employed for the achievement of

In principle, the appropriate quantity of basic research is that which will cause the rate of return on investment in that activity to be the same as the rate of return on investment in other lines, adjusted for risk; it should be not less than the rate of interest. The rate of return on investment in private economic activities in the market indicates the return to be sought from public investment. The application of the rule would require the estimation of the cost of basic research, of the gains from the increments of knowledge it produces, of risk and of discount rates. It is an extremely difficult if not impossible rule to apply explicitly and for this reason the task of applying it should be sedulously avoided by governmental officials. To the extent that they can, they should permit the market to allocate resources to research and among research fields, except where a strong case can be made that market failure will occur.

The nature of the difficulty in measuring the economic benefits of basic research can be seen in a paper prepared for a Working Group of the Council on Scientific Policy of the United Kingdom under the chairmanship of the late Professor Harry Johnson.[12] That paper suggests that the value of future discoveries of basic research might be measured by examining the value of the contributions to science-based industrial practice which basic research has made in the past. Such a measure would not, however, go far enough, for discoveries that never have applied uses also have value since they might add to society's knowledge of nature. Lord Robbins once said, "We do not say that the production of potatoes is economic activity and the production of philosophy is not. We say rather that, in so far as either kind of activity involves the relinquishment of other desired alternatives, it has its economic aspect."[13] The cost of philosophy can be calculated; the benefits of philosophy are incalculable. On what grounds can one decide how much philosophy should be paid for? The same applies to basic research which does nothing less nor more than add to our understanding of nature.

If the contribution of any field of basic research to the more profound understanding of nature were a constant or if their magnitudes were known, the ultimate contribution to industry would be a sensible base for the estimation of the whole benefit from basic research. This seems to be those purposes. The solution of the problem of arranging the appropriate scale is less difficult in some of these cases than in others. The gains to society in persons saved from death or injury and property saved from destruction and damage from the construction of lighthouses and levees and gains from the cheapened costs of transportation from the dredging of rivers can be relatively easily estimated. It is, however, much more difficult to estimate the more amorphous gains from the building of monuments, the setting aside of conservation areas, and the production of increments to the stock of pure knowledge about nature. How is one to measure the values to society of somewhat deeper understanding, for their own sakes, of the emission of energy by quasars, immune processes at cellular and molecular levels, the synthesis of polymers from non-petroleum resources, the electronic structure of an ordered solid, and the processes of chemical migration in the earth's crust in comparison with the values of known commodities and services to the production of which resources employed in research might have been put?

[5] National Science Foundation, *The Five-Year Outlook: Problems, Opportunities, and Constraints in Science and Technology* (2 vols., Washington, D.C.; Government Printing Office, 1980).

[6] *Ibid.*, vol. II, p. 464.

[7] *Ibid.*, p. 465.

[8] *Ibid.*, p. 467.

[9] *Ibid.*, p. 467.

[10] *Ibid.*, p. 467.

[11] *Ibid.*, p. 464.

an heroic assumption; some fields of basic research probably have closer and more immediately realised relevance in technology than do others. If, however, the various fields were aggregated and those from which large and quick bits of unintended, applied uses fall out from the pure knowledge produced bv work in them are summed to those from which applied uses are small and slow in coming, or never occur at all, an average estimated social value of the industrial contribution of pure knowledge could be construed. In order to know the appropriate scale of the use of society's resources for basic research, one would still need to add on some increment of value for the addition to the social stock of pure knowledge. That "commodity" is not traded in markets so we cannot observe a set of prices that would define its value for us. The resources that are employed in its production are market-traded and do have prices that denote their value but it would not be correct to infer the value of pure knowledge from the value of the resources employed in its production, in the same way as the value of the services of government is, indeed, currently measured by those who construct national income accounts. The value of the output of basic research is sought precisely in order to determine the quantity of resources that would be optimally used to produce pure knowledge. If such a procedure were applied, the case would be made in circles. The value of the resources used in basic research would then be said to represent the value of its output. On the basis of this tautology, any scale of basic research would become defensible.

Since it does not seem possible to measure the value of pure knowledge of nature that has no intended applied use, economic principles of calculating the optimimum offer very little practical guidance in determining the quantity of society's resources which should be devoted to basic research.

Even if the value of pure knowledge and other relevant variables could be estimated, so that the appropriate scale of basic research could be determined, other problems would still persist in estimating the portion of basic research that should be subsidised by the federal government. It is not true that, in the absence of subvention, no basic research is done at all. It is only that, because pure knowledge is a public good, the market would cause underinvestment in basic research. To know how much subvention were required, one must know not only the appropriate social scale of basic research, but also the quantity of it that would be done without the deliberate intervention of government. In addition, there are forms of subvention which occur, other than the subventions by the government which aim explicitly to support basic research. Scores of thousands of scientists who are employed by universities undertake scientific and scholarly work, in addition to giving instruction to their pupils. That scientific and scholarly work is often basic research. The universities of these scientist-scholar are supported in part by the tuition fees of students, by philanthropic gifts and income generated by the investment of those gifts and, in the case of public universities, by the appropriation of funds by legislatures that cover the costs of the universities above the revenues which they receive from tuition fees of students and from other sources. Thus, some subvention of basic research occurs in the absence of an international and explicit programme of support for that activity. The determination of the appropriate scale of direct governmental support for basic research would need to take into account the quantity of basic scientific research that would go on even without that direct support. If that were not done, a government's programme of support might be too large, so that an excess of resources would be employed in basic research. Alternatively, federal subsidies might simply constitute transfer payments, allocating no more resource to basic research than would be devoted in such activities, even if there were no federal governmental programme of support. Governmental subsidies for basic research would then merely permit the receiving institutions to withdraw some funds from basic research to use them for other purposes, or the subsidies might take the form of incremental income for basic research investigators who would perform the same tasks that they would have undertaken, even if there had been no direct governmental subsidy.

United States Science Policy

Actual United States governmental support for scientific and technological research is not confined to basic research. Government has extended its influence beyond its proper limits.

Research and development expeditures in the United States in 1975 were larger than the combined expeditures for that purpose in the United Kingdom, France, West Germany and Japan.[14] Those countries, together with the Soviet Union, are said to do about 90 per cent. of the research and development done in the entire world. American research expenditure is relatively larger than expenditure or research in other countries when adjusted for population, employment, and gross national product.

In 1980, 2·3 per cent. of resources employed in all economic activities in the United States, as measured in the gross national product, were devoted to research.[15] In that year, $60·4 billion were spent on research and development. The work was done in governmental research institutions, at private industrial firms, universities and colleges, centre administered by consortia of universities, and at other institutions not intended to make a profit.

Industrial firms did the largest amount of research and development and most of the work done in industrial firms was financed by their own funds. Of the total expenditures in the United States for research and development of $60·4 billion in 1980, $42·3 billion were spent by private industrial establishments and $28·3 billion of the latter sum were supplied by industrial firms for themselves.

Of the total of $60·4 billion, $38·6 billion were spent for development, $13·5 billion for applied research and $8·2 billion for basic research.[16]

The influence of the federal government on the scale and composition of the country's research and development enterprise is very large. An active public science policy in which large government expenditure for research occurs is a phenomenon of the period after the Second World War. In 1940, federal expenditure for this purpose was very small and it was mainly for research relating to national defence and agriculture. By 1980, the federal government financed about one half of all research and development carried on in the country through supporting governmental laboratories or by making grants for research to other institutions or contracting to have it done at other institutions.[17]

About one half of federal governmental expenditures for research and development is for national defence. Major expenditures for other purposes were in the fields of space, health, energy developments and conversion, environment and transportation and communication. Governmental expenditures for research and development are distributed as follows: 63 per cent. to development, 24 per cent. to applied research, and 13 per cent. to basic research. Federal government funds financed about 70 per cent. of all basic research that is done in the United States and about 50 per cent. of all applied research that is done.[18] (The authority to engage in, to contract for, or to make grants for scientific and technological research is widely diffused among numerous departments and agencies of the federal government. The Department of Defense makes by far the largest expenditures for this purpose. Large expenditures are also made by the National Aeronautics and Space Administration, the Department of Energy, the Department of Health and Human Services, the National Science Foundation, the Environmental Protection Agency, and the Departments of Agriculture, Transportation, Interior and Commerce.

[14] Byatt, I. C. R. and Cohen, A. V., *An Attempt to Quantify the Economic Benefits of Scientific Research*, (Science Policy Studies No. 4), (London: H.M. Stationery Office, 1969).

[15] Robbins, Lionel, *An Essay on the Nature and Significance of Economic Science*, 2nd edn. (London: Macmillan, 1935), p. 17.

The government is deeply engaged in the support of applied and technological research. Its support enlarges the scale of those enterprises and greatly affects the kinds of applied and technological research work that is done, and the institutions in which it is done.

Where government produces a public good, such as national defence, from which none can be excluded as a beneficiary, and government is the sole purchaser of a particular kind of commodity or service, it is appropriate that government itself produces or purchases a relevant applied or technological research service. If the government is the sole purchaser of military submarines, it is proper that it contract for the purchase of research that will improve the quality of sonic detecting devices installed in submarines. The national defence establishment is the sole purchaser of such research; very little of it would be produced, in the absence of a defence contract or unless it is done in a governmental research establishment.

The argument for governmental support for applied and technological research on the grounds that the government is the sole purchaser must, however, be employed with care. There are limits to its proper use. Suppose, for example, that a large number of municipalities supply water to their householders. Agents and devices for the purification of water are purchased by the municipal water authorities from private firms that sell water system supplies. The discovery of new knowledge through research that will permit the manufacture of new and better agents and devices for purifying water will permit the discoverer to acquire the profit from his discovery in the sale of the knowledge to the supplying firms. The market would not fail in this case. It would arrange the use of the optimal quantity of resources in such ventures in research. Where there are many firms supplying many agencies of government, the risks of research are not extraordinarily large. If some cities do not install new water purifying systems, others might. That governments are, ultimately, the sole purchaser of water purifying agents does not imply that government should support the production of applied knowledge about water purification.

Most of the applied and technological research which the non-military governmental agencies subsidise or produce directly does not have the property that government is a sole purchaser. The decisions to have such research done were centrally determined by the public agencies. The market has not been permitted to act as the instrument for deciding whether such applied and technological research work should be done and, if so, how much of it, and which institutions should do it. Where there are externalities, with divergences between private and social benefits from applied scientific and technological research governmental subsidy is appropriate; but much of the subsidised work clearly did not have that feature.

Flaws in United States Science Policy

The National Science Foundation offers, in its first annual report on science and technology, published in 1978, its argument for active intervention by government in scientific and technological research. "The market mechanism by itself", it says, "is likely to lead to an underinvestment in research and development from society's point of view."[19] The foundation explains that those who engage in some kinds of research work are unable to appropriate the benefits of their work, and that the private sector is sometimes unwilling to assume the high risk and is unable to assemble the large quantity of capital required in some socially beneficial ventures.

The foundation is certainly correct in its observation about the unappropriability of the results of some scientific discoveries. It is incorrect when it implies that the intensity of aversion to risk of the makers of science policy is socially preferable to market indications of the community's aversion to risk. It is also incorrect that the private sector cannot assemble capital of magnitudes necessary for large scientific undertakings. There are many examples—large telecommunications systems and offshore oil drilling, for instance—of private aggregation of very large quantities of capital.

A large fraction of governmental projects in science and technology do not meet the standard of unappropriability that gives warrant to government participation in research in the fields in question. If we find that governmental science policy subsidies research in non-public goods where there are no externalities, we may conclude that the public sector should not be there, that governmentally supported science is conducted on too large a scale and that it should be reduced to avoid waste of resources.

Such cases are not hard to find.

For example, the Department of Energy announced, in 1980, a National Passive and Hybrid Solar Energy Program of aggressive research, development and market penetration.[20] The programme includes development of cost competitive marketable passive solar heating designs, systems and products for residential and commercial structures, passive cooling technologies and daylighting systems.

The exercise of monopoly power by the oil-producing countries, which has driven up prices of fossil fuels, is familiar to builders, architects, engineers, tool and material makers; it is well known to those who sell heating, cooling, ventilation, hot water, and lighting services; to those engaged in hothouse agriculture; and to consumers of shelter. Producers have an incentive to find inexpensive, attractive, and efficient substitutes for fossil fuels; their earnings will rise if they do. Consumers have an incentive to substitute other fuels; they will reduce their costs if they do.

In this case, no externalities exist. The costs of the search for and discovery of alternatives to fossil fuels would fall wholly upon those who invest in search and discovery; the financial gains from the discovery of alternatives can be wholly obtained by those who incur the cost of discovery. Persons and firms which are not prepared to pay the cost of discovery, can be excluded. Clearly, the market should be permitted to decide whether of not to devote resources that have other values to society to the discovery and design of passive solar systems. It can also decide how much of its resources to devote to that purpose. Government has no useful role to perform in this case.

If governmental decisions subsidise research which would not have been undertaken through the market, or if the research is greater than it would be if the forces of the market had their effect, the community is ill served by governmental policies for scientific and technological research. The Department of Energy under these conditions, is putting society's resources to wasteful uses.

The passive solar energy case illustrates what is a widespread occurrence in governmental expenditures for science and technology. The public authorities operate in areas in which the social judgment of the market would give better results. Scientists and technologists are diverted into

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[14] National Science Foundation, *Science Indicators 1978, Report of the National Science Board 1979* (Washington, D.C.: Government Printing Office, 1979), p. 6.

[15] National Science Foundation, *National Patterns of Science and Technology 1980* (Washington, D.C.: Government Printing Office, 1980), p. 29.

[16] *Ibid.*, pp. 26, 27, 28.

[17] *Ibid.*, p. 25.

[18] *Ibid.*, pp. 30, 31.

[19] National Science Foundation, *Science and Technology: Annual Report to the Congress, August 1978* (Washington, D.C., Government Printing Office, 1978), p. viii.

[20] Department of Energy, Office of Solar Applications in Buildings, *Passive and Hybird Solar Energy Program Update*, September, 1980, duplicated.

less productive work than they might otherwise do. Governmental expenditures of this kind should be eliminated and tests of the failure of markets should be applied more rigorously. These public funds could then be turned to better purposes.

The first *Five Year Outlook: Problems, Opportunities and Constraints in Science and Technology*, published in 1980, is full of similar examples.[21] Agencies with funds to spend in support of research list their preferences and their aspirations in that report. The Department of Commerce, for example, wants a new generation of very large cargo-carrying tankers to achieve goals of safety, dependability and productivity.[22] The reduction in oil spills presumably would make more likely the construction of additional petroleum-refining plants and storage capacity on the Atlantic Coast. This department also wants phase-diagrams and the properties of multicomponent alloys to be investigated so that replacements can be found for the scarce metals used for alloying. It wants to find composite material of higher strength, lighter weight and greater durability than conventional materials. It wants waste of materials to be diminished by developing or improving processes for the recovery of resources. It wants to know more about ways to produce and preserve sea-food, and it wants new technology for the production of fish through aquaculture.

The Department of Health and Human Services wants to find a blocking agent to treat the victims of overdoses of drugs.[23] The Department of Energy wants research on advanced catalysts for liquefying and gasifying coal.[24] The Department of Transportation wants research on applications of micro-electronics to the automobile, on pipeline corrosion control, on automated systems to improve the circulation of the populations in the centres of cities, and on the development of materials to replace asphaltic concrete and portland cement in the construction and repair of highways.[25]

These are clearly cases of bureaucratic distension. It would be good to have all these things if they came at zero cost. They do not. They consume resources which have other uses, so they imply opportunity costs. They should be sought only if the investment in the search for them yields a satisfactory return, adjusted for risk. We cannot know whether or not to seek them except by applying the test of the market. If the market recommends such investments, they ought to have been sought out. If the market does not do so, they are socially inappropriate.

The various desiderata enumerated in *The Five Year Outlook* are not public goods. There is no problem of the incapacity of investors to acquire the whole gain they would produce. There is no problem of exclusion and there would be "free riders". There are no externalities.

In these cases, social and private costs and gains would not diverge in the market. Incentives for socially appropriate research exist in the market. In cases such as these, government intervenes mischievously.

Having penetrated the fields of applied scientific and technological research by making decisions for the support of research that should properly have been left to the processes of the market, the federal government undertook also, in the period of the Carter administration, to initiate a "commericialization" programme that would increase the rate at which discoveries made through research are employed in industrial and commercial practice.[26] The grounds for overriding the market by government subvention are indeed tenuous. The government said it has a "national perspective" which is absent from the perspective of private industry and it said it has a "unique role in serving social needs".[27] There is in these defences for government commercialisation programmes a lack of understanding of the capacity of the market to compose social preferences by aggregating individual preferences.

Making correct explicit decisions about the quantity of resources to be devoted to scientific and tehnological research and about the fields, projects, and institutions among which those resources should be distributed is so difficult that the public officials should avoid the task as much as possible and let the decisions be made in the market. Public goods should be narrowly defined; there should be a certain reluctance to find externalities or, where they seem to exist, but are of trivial magnitudes, they should be ignored.

Instead the federal government has pursued an active role in the support of scientific and technological research; responsibilities which are difficult to carry out have been assiduously sought out; market failures seem to have been found with great frequency by the simple affirmation of the officials.

Once the government has chosen appropriately the kinds of scientific and technological research which it should support by searching out public goods and markets in which the existence of externalities cause private underinvestment in research to occur, the government then confronts the problem of allocating resources among research fields, among institutions at which the research work will be done, and among persons who will conduct the research.

The processes by which governmental decisions have been made in the United States on the uses to which resources for scientific and technological research would be put have been complex and convoluted. Such processes of decision-making have been followed both in the research areas in which government intervention is appropriate and in those in which it is inappropriate.

Allocative outcomes ensue. Some examples follow. Some $31·0 billions of resources were allocated by the federal government in 1980 to research and development rather than to other purposes. Of $11·8 billions of that sum which were allocated to research, as distinguished from development, 62 per cent. was allocated to applied research and 38 per cent. to basic research. Of basic research resources, 24 per cent. was allocated to work done intramurally in federal governmental laboratories by scientists and technologists who were employees of the government and 76 per cent. was for work done in other institutions under contract or with governmental grants. Of resources allocated to basic research, 45 per cent. was allocated to work in the life sciences, 30 per cent for work in the physical sciences, and lesser proportions to work in the environmental sciences, engineering, the social sciences, mathematics and computer sciences, psychology, and other sciences.[28] Within the physical sciences, 52 per cent. was allocated for basic research in physics, 25 per cent. in astronomy, and 22 per cent. in chemistry. Within the environmental sciences, 37 per cent. was allocated for basic research in atmospherics, 35 per cent. in geology, and 25 per cent in oceanography.[29] Of the resources for research and development that were available to the National Institute of Health in 1980, 30 per cent. were allocated to research on cancer, 15 per cent. to the heart, lung and blood, 10 per cent. to arthritis, metabolism, and disgestive diseases, 7 per cent. to neurological and communicative disorders and strokes, 6 per cent. to allergy and infectious diseases, 6 per cent. to child health and human development, 3 per cent. for research in connection with the eye, and the remainder to a variety of other purposes. Of the resources allocated by the National Institute of Health for work on the eye, 40 per cent. was allocated to retina and choroidal diseases, 19 per cent to sensory motor disorders and rehabilitation, 13 per cent. to corneal diseases, and about 10 per cent. to work on glaucoma and cataracts.[30] Of resources allocated by the federal government to research, both basic and applied, in 1980 for research to be done outside the governmental agencies themselves, 45 per cent. was allocated for work to be done at universities and colleges, 28 per cent. to be done by industrial firms, and the remainder at other institutions.[31] Resources for research and development in 1980 were heavily allocated for research to be done in the states of California, Maryland, Massachusetts, New York, Ohio, Florida, and Pennsylvania; lesser quantities of resources were allocated for work done in the other states. Research and development to be done in California was 158 times greater than that to be done in Kentucky.[32]

From one time to another, changes occur in the federal government's allocation of resources for scientific and technological research among various purposes. From 1969 to 1974 federal government expenditures for research and development for all purposes rose at an annual rate of 2·2 per cent.; from 1974 to 1977 the annual rate of growth of those expenditures was 11·2 per cent. Expenditures for space research and development declined at an annual rate of 7·8 per cent. in the former period and rose at an annual rate of 7·4 per cent. in the latter period. Expenditures for research in education rose at an annual rate of 2·3 per cent. in the earlier period and declined at an annual rate of 11·5 per cent. in the later one. Expenditures for research on health rose at a 13·2 per cent. rate from 1969 to 1974 and at 7·5 per cent. rate in the period 1974–77. Research on food, fibre and other agricultural products first rose at a rate of 5·3 per cent. and then at a rate of 16·3 per cent. Research and development related to energy rose in the period 1969–74 at a rate of 8·8 per cent. annually and at a rate of 51·2 per cent. from 1974 to 1977.[33]

In 1970, $12 million of resources were employed in research and development in connection with the space shuttle; in 1980, the corresponding number was $1,886 million.[34] Between 1969 and 1980, expenditures for cancer research rose twice as rapidly as expenditures for all biomedical research.[35] Research and development expenditures on coal were 36 times greater in 1980 than they were in 1969, although expenditures for all energy research and development rose in that period only by a factor of eight.[36]

Decisions on the allocation of resources for research finally emerge after a complicated set of influences plays upon the processes of decision. Decision-making is not centralised within the government.

Some agencies of government, such as the National Science Foundation and the National Aeronautics and Space Administration, are almost wholly and explicitly devoted to supporting research. Other agencies, such as the Department of Agriculture and Transportation, are mainly dedicated to other purposes but they have funds to spend for research.

The Political Element in Governmental Allocation

Congressional committees and individual members of the congress affect the pattern of expenditures, as does the White House, which is sometimes active and sometimes passive in expressing its preferences. Scientific advisers to presidents and scientific advisory boards come and go, rotate their memberships, and exercise more or less influence. Governmental agencies which oversee government spending affect the outcome, as do associations of university teachers and of practitioners in the various disciplines, and influential laymen who have concentrated their interests upon narrow objectives or who seek to have work that is

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[21] National Science Foundation, *The Five-Year Outlook.*

[22] *Ibid.*, vol. II, pp. 325ff.

[23] *Ibid.*, pp. 371ff.

[24] *Ibid.*, pp. 361ff.

[25] *Ibid.*, pp. 437ff.

[26] National Science Foundation, *Science and Technology: Annual Report to the Congress, June 1980* (Washington, D.C.: Government Printing Office, 1980). See chapter VIII, "Fundamental considerations on the government role in the development and commercialization of technology", pp. 53-60.

[27] *Ibid.*, p. 55.

[28] National Science Foundation, *Federal Funds for Research and Development, Fiscal Years 1979, 1980, 1981* (Washington, D.C.: Government Printing Office, 1980), p. 14.

[29] *Ibid.*, p. 63.

[30] National Science Foundation, *Federal R & D Funding by Budget Function, Fiscal Years 1979–81* (Washington, D.C.: Government Printing Office, 1980), p. 22–23.

[31] National Science Foundation, *Federal Funds for Research and Development, Fiscal Years 1979, 1980, and 1981*, p. 34.

[32] *Ibid.*, p. 137.

subsidised by the government done in the cities and regions in which they reside.[37]

This welter of influences affects both the aggregate size of the public-sector "science effort" and the relative sizes of the constituent parts. At various times, emphasis has been given to oceanographic research, to space exploration, to "wars" on cancer or poverty, to environmental improvement, to fossil fuel substitution, and to the boring of the earth's crust and mantle. Political influences loom large in these judgements. Hierachies of status and financial allocations have been determined by idiosyncratic presidential interests, by hopes of marginally increasing the probability or re-election, and by attempts to improve the relative position of colleagues in a particular discipline. Congressmen and other political figures stake out claims for the location of publicly financed scientific activity in their own constituencies. Scientists and academic administrators use a variety of political strategies in their competition with other claimants.

Governmental support for research in marine science and technology was turned, in the Johnson administration, to work on the development of ground fish meal and on coastal weather prediction because "[President] Johnson's self-image as a humanitarian made a fish-concentrate proposal to feed a hungry world seem attractive [and] Johnson's Texas background allowed him to appreciate the destructiveness of hurricane and tornadoes and the need for improved prediction along the coasts."[38] Support for biomedical research on heart ailments was accelerated because President Johnson had suffered from cardio-vascular illness.[39] Presidential counsellor Theodore Sorenson instructed a member of the staff of the office of the science advisor to President Kennedy to develop proposals for expanded research in oceanography; he "felt that oceanography would be particularly appreciated by Kennedy because of his love for yachting and his experience aboard PT boats."[40]

Government expenditures for research on the treatment of cancer were expanded in the Nixon administration as a result of "the personal intercession of Elmer Bobst, a close friend of the President" and of Mary Lasker,[41] a philanthropist with a concentrated interest in the progress of cancer therapy. President Eisenhower recommended to the Congress the appropriation of funds for the construction of the Stanford Linear Electron Accelerator, reflecting "the fact that some physicists had the trust of the White House and, further, they used this confidence to support their own programmes, even at the expense of other scientists (and most directly, other physicists)." To bring about this outcome, there was "assiduous lobbying by elements of the scientific elite".[42] President Eisenhower withdrew from his intention to veto expanded congressional appropriations for biomedical research because "Mary Lasker . . . was good friends with Jules Stein who in turn had a good friend who often played golf with Eisenhower, and auspiciously was scheduled to play with the president in Newport. As a favour to Stein, this friend agreed to help convince the president of the dire consequences for national health should the budget be vetoed."[43]

A coalition of universities of the mid-western states organised a delegation of congressmen to confer with President Johnson in order to promote the cause of the construction of a high-energy accelerator at Madison, Wisconsin. At that meeting, the late Hubert Humphrey, then a member of the Senate, exclaimed, "Why, my God, the Midwest has been getting shortchanged."[44] Scientists energetically lobbied the Congress seeking to advance this cause.[45]

A more uniform geographic distribution of federal research expenditures is frequently advanced in the Congress as are claims for the location of particular research activities in the constituency districts of given members of the Congress. An amendment to the Authorisation Act of 1965 of the National Aeronautics and Space Administration said, "It is the sense of Congress that it is in the national interest that consideration be given to geographic distribution of federal research funds whenever possible." The author of the amendment was the then Senator Walter Mondale who said, during the discussion, "The position of those placing [NASA] contracts asserts that their policy is to put the money where the competence is. I think there are some fallacies in that position."[46] In the 1960s, the National Science Foundation altered its policies on the award of fellowships in order to achieve a more proportionate geographical distribution. In an attempt to reduce the concentration of federal support of scientific research in relatively distinguished institutions and in response to President Johnson's interest in "geographical balance", the Foundation administered a programme of more intensive support for "second-tier" universities which was intended, it was said, to "get more institutions into the top 20".[47]

When principles of choice such as these come to have a weighty influence upon the allocation of resources to scientific and technological research and among research fields, projects and institutions, it seems clear that society's resources will be put to wasteful uses. Parochial interests and low standards come to dominate allocative choice and the process of scientific assessment and the exercise of scientific judgement no longer plays its proper role. Inappropriate tests are applied, wrong outcomes are generated, the scientific community comes to be governed by the wrong standards, and the growth of scientific knowledge hampered.

An Alternative Allocative Method for Research

It has been suggested that, except in the cases of basic research and of applied research in which where there are definite and large externalities, government should not intervene. With those exceptions, commercial and industrial markets should be permitted to determine judgement on whether a particular piece of research should be undertaken, how much research should be done, in which fields, projects and institutions it should be done and the methods by which it is done.

For basic research, for which some governmental intervention is appropriate, explicit calculation of the proper scale is extremely difficult because governmental agencies, like any other institution or individuals, lack the capacity to measure the value of the output of pure knowledge; they do not know in advance what will be discovered by those who are engaged in research; they do not know how long it will be before a research

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[33] National Science Foundation, *An Analysis of Federal R & D Funding by Function* (Washington, D.C.: Government Printing Office, 1980), p. 4.

[34] *Ibid.*, p. 40 and National Science Foundation, *Federal R & D Funding by Budget Function, Fiscal Years 1979–81*, p. 38.

[35] National Science Foundation, *An Analysis of Federal R & D Funding by Function*, p. 35 and National Science Foundation, *Federal R & D Funding by Budget Function, Fiscal Years 1979–81*, p. 10.

[36] National Science Foundation, *An Analysis of Federal R & D Funding by Function*, p. 37 and National Science Foundation, *Federal R & D Funding by Budget Function, Fiscal Years 1979–81*, p. 22.

[37] A large literature has grown up on the political element in science policy in the United States. See, for example, Katz, James Everett, *Presidential Politics and Science Policy* (New York and London: Praeger 1978); Penick, J. L., Jr., et. al (eds.), *The Politics of American Science, 1939 to the Present* (Chicago: Rand McNally 1965); Strasser, Gabor and Simons, Eugene M., (eds.), *Science and Technology Policies, Yesterday, Today, and Tomorrow* (Cambridge, Mass., Ballinger, 1973); Golden, William T., (ed.), *Science Advice to the President* (New York; Pergamon, 1980); Regan, Michael D., *Science and the Federal Patron* (New York; Oxford University Press, 1969); Greenberg, Daniel S., *The Politics of Pure Science* (New York; The New American Library, 1967); Primack, Joel and von Hippel, Frank, *Advice and Dissent, Scientists in the Political Arena* (New York; Basic Books, 1974); Dupré, J. Stefan and Lakoff, Sanford A., *Science and the Nation, Policy and Politics* (Englewood Cliffs, N. J. Prentice-Hall, 1962); Piccard, Paul J., (ed.), *Science and Policy Issues* (Itasca, Ill.; F. E. Peacock, 1969); Jachim, Anton G., *Science Policy Making in the United States and the Batavia Accelerator* (Carbondale and Edwardsville, Ill., Southern Illinois University Press, 1971). There is also an immense literature on this subject in various journals of political science and science policy.

[38] Katz, J. E., *op. cit.*, p. 160.

[39] *Ibid.*, p. *161*.

[40] *Ibid.*, p. 131.

[41] *Ibid.*, p. 198.

[42] *Ibid.*, p. 108.

[43] *Ibid.*, p. 112.

discovery occurs, how long the discovery will survive before it becomes intellectually obsolete and the rate at which it is appropriate to discount the services the discovery performs in giving society deeper understanding of aspects of nature. Although a governmental programme of support for basic research that will move some resources that would otherwise have been employed in other activities into basic research is probably called for, care should be shown that such a programme is not too large.

Just as government lacks the capacity to measure the value of increments of pure knowledge in the aggregate, it also lacks the capacity to measure the relative values of different increments of pure knowledge that are produced by basic research work in different fields. It cannot explicitly rank the increments of knowledge of the constituent components of basic research nor can it assign them weights that indicate their relative value to society. Government should avoid, if it can, tasks of measurement that it cannot carry out. There is, however, a method which it can employ that might solve the problem of allocating governmental funds for support of basic research, while avoiding explicit estimation of the values of the output of research activities. That method implies choosing among scientists rather than choosing among scientific fields and projects.

There is a kind of intellectual market within which scientists work. The late Professor Michael Polanyi described it when he wrote:

> The community of scientists works according to economic pricinples similar to those by which the production of material goods is regulated. . . . [T]he activities of scientists are in fact coordinated. . . . This consists of the adjustment of the efforts of each of the hitherto achieved results of the others. We may call this a coordination by mutual adjustment of independent initiatives . . . [which] leads to a joint result which is unpremeditated by any of those who bring it about. Their coordination is guided by "an invisible hand" towards the joint discovery of a hidden system of things. . . . Any attempt to organise the group. . . . under a single authority would eliminate their independent initiatives and thus reduce their joint effectiveness. . . . It would, in effect, paralyse their cooperation. . . . [T]he pursuit of science by independent self-coordinated initiatives assures the most efficient organisation of scientific progress. . . . [T]he scientist responding directly to the intellectual situation created by the published results of other scientists is motivated by current professional standards. . . . [T]he decisions of a scientist choosing a problem and pursuing it to the exclusion of other possible avenues of inquiry may be said to have an economic character. For his decisions are designed to produce the highest possible result by the use of a limited stock of intellectual and material resources.[48]

In an intellectual market such as this, the scientific community exercises scientific standards in the determination of scientific merit. The authority of scientific opinion enforces scientific standards. All independent scientists participate in the administration of the authority of scientific opinion.

Scientific standards are, Polanyi asserts, uniform throughout science. This "makes possible the comparison between the value of discoveries in fields as different as astronomy and medicine. This possibility is of great value to the rational distribution of efforts and material resources throughout the various branches of science . . . [and it is] the principle which underlies the rational distribution of grants for pursuit of research. . . . So long as each allocation follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole."[49]

"Intellectual markets" are like commercial markets in another sense that Polanyi does not touch upon. If commercial markets are efficient instruments for deciding the uses to which resources should be put to serve society's purposes, it is because there is competitive bidding for resources among those who seek to employ them and there is freedom to enter and depart from industries and occupations. Competitive bidding and the freedom to enter and depart are also properties of intellectual markets. Young persons freely choose whether they will train in chemistry or physics; if physics, they freely choose whether they will specialise in spectroscopy or cosmic rays; if one or the other, they freely choose once they reach a certain level of seniority which research projects they will venture upon and which they will abandon. In commercial markets, efficient firms bid away resources from inefficient firms, so that resources are put to more highly valued uses. In intellectual markets, creative scientists of outstanding accomplishments will be bid for by institutions which are interesting intellectual communities with large complements of scientific workers where they will be able to participate in stimulating dialogue and on higher levels, be nourished by others with original minds and be provided with opportunities for serious work in their chosen fields. Those with less talent and more mediocre minds find themselves in less professionally attractive places. As in commercial markets, intellectual resources are put by the intellectual market to their most highly valued uses.

resources for basic scientific research which would permit governmental officials to avoid the explicit calculation of costs and gains. They would allocate not among fields but, rather, among scientists. They would inquire about the qualities of intelligence and of the professional achievement or promise of scientists in the queue, but would not ask about the relative values of the various and different discoveries of pure knowledge they seek to achieve. They would not seek to re-fashion the structure of pure scientific work but would permit its structure to emerge spontaneously from the unconstrained choices of individuals among scientific careers and scientific projects.

In choosing among applicants for research grants, the scientific assessment of professional merit would be consulted. This might be done directly by governmental agencies or they might do so indirectly by making block grants to the relatively small number of distinguished universities[50] and by having those universities re-allocated those grants among scientist-applicants. The universities might be permittted to make grants for the support of scientific research to individuals located at other institutions, as well as at their own, and they would be permitted to consolidate some of the funds provided to them with those of other institutions so that consortia could be formed for the construction of expensive capital facilities such as those required for research in certain branches of astronomy and physics.

By applying the method of searching out scientists who have merit, as measured by the consensual judgement of the scientific community, and by permitting them to work on problems that rouse their curiosity and are responsive to their sense of what the scientific community considers to be important, the government will make a more valuable contribution to the advancement of science and the progress of knowledge than it would if it attempts the explicit estimation of the relative values of scientific discoveries or if, as now, allocations are decided in response to parochial and political influences.

Allocation among Fields and the Scale of Basic Science

The criteria of choice for governmental support of scientific and technological research which are suggested in this paper are in some respects the same, and in other respects different, from those suggested by Dr. Alvin Weinberg in his fundamental paper on the criteria for scientific choice.[51] The resources of society that are devoted to research must be allocated, Weinberg writes, among "often incommensurable fields of science—between, for example, high-energy physics and oceanography or between molecular biology and science of metals". He proposes some criteria that are internal to the scientific field and which deal with the question: how well is the science done?, and other criteria that are external to the field and answer the question: why pursue this particular science? The internal criteria may be analysed into questions about the adiness of the field for scientific advancement and about the competence the scientists who work in the field. The external criteria may be analysed into technological merit, which depends upon the practical uses of the knowledge the scientific research will produce, scientific merit, which depends upon the contribution that work in one scientific field makes to the progress of knowledge in other scientific fields and social merit, which depends upon how well research will serve the objectives which government and society seek to achieve.

I argue here that if research has intended practical uses, it should not be supported by governmental agencies, except where there are market externalities or where a single governmental agency—or a small number of governmental agencies—is the sole purchaser of the result of the research. Research that is intended to be applied practically in the production of commodities and services should be required to pass the test of the market in deciding whether it should be undertaken. Weinberg's criterion of technological merit should not, therefore, be applied by government but should be left to the market.

Nor, with the same exceptions, should the criterion of social merit be employed in the allocation of governmental funds for research. There are a large number of governments and private persons in the market for effective contraceptive agents, longer-lasting foods which resist spoilage, crops which are disease-resistant and higher-yielding and which require fewer fertilisers and less water for their production, and therapeutic and immunological agents that will diminish the incidence and duration of illness. That governments are concerned with controlling the rate of population growth, increasing food supplies, and reducing the rates of morbidity and mortality does not lead ineluctably to the conclusion that governments should subsidise research serving those objectives. If some of society's resources should be devoted to such research, private investment in the research will yield a rate of return that will be high enough to assure that the research will be done. There is no efficient way to know how many of those resources should be put to those purposes except by applying the test of the market.

Governmental support is indicated, however, for research in pure science seeking increments of pure knowledge about nature which are not intended, nor explicitly foreseen, to have practical use. Weinberg's internal criteria and the criterion of scientific merit in his set of external criteria are proper and correct for the productive allocation of resources for research of that kind. The scientific competence of the applicants should be examined as should be the scientific promise of the work they propose to do. The allocation of resources among fields need not be explicitly confronted. In their research, desire to make discoveries which the scientific community will consider to be important and creative, to increase the understanding of nature and to contribute to the progress of knowledge. The problem of allocating resources for research among fields need not be solved directly; one need only observe the rule making allocations among scientists by applying the test of scientific competence.

If government is to support basic research, what should be the scale of that support? What part of the resources of society should the government bid away from other activities that are useful and productive in order that the resources be given over to basic scientific research? The determination of the optimal scale of basic research is extraordinarily difficult. There are

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[48] Polanyi, Michael, "The Republic of Science", in *Minerva*, (1962), pp. 54–73.

[49] *Ibid.*, pp. 60–61

[50] For a suggestive identification of the distinguished universities, see Rouse, Kenneth D. and Andersen, Charles J., *A Rating of Graduate Programs*, (Washington, D.C.: American Council on Education, 1970). This publication reports the results of a survey of university teachers who were asked to rank American universities with regard to the quality of their graduate programmes for each of a large number of disciplines

[51] Weinberg, Alvin M., "Criteria for Scientific Choice", *Minerva*, I (1963), pp. 159–171.

no market measures of the value of society to increments of pure knowledge and, thus, no standard for comparing that value with the values of things that resources used for research might have produced, if they were put to alternative uses.

Dr. Weinberg has suggested that the scale of governmental support for basic science should be equal to the sum of some fraction of the expenditures of society for applied research, because basic research produces discoveries which, in the end, cause applied research to be more fruitful, and some fraction of the value of "society's entire technical enterprise", because basic science contributes to "the technological system as a whole".[52] "Society's entire technical enterprise" is assumed to mean the whole output of commodities and services of the economy in some period—say in a year—or what the economists call the "national product".

Dr. Weinberg does not say what the appropriate fractions should be, except to suggest that they would be politically determined. This offers no guidance to the politicians on the question of size of the fractions which would be optimal for society. To determine the scale of basic science by some measure of its practical value is, however, fundamentally defective. To the extent that basic science has practical value—for example, by improving commodities and services, cheapening the cost of their production, and improving the material standard of life of the people—the appropriate scale of basic science will be best determined by the market. That part of basic science should have no governmental support. It is the part of basic science that has no practical use and serves only to enlarge and improve the knowledge and understanding of nature that warrants governmental subsidy.

The criteria for determining its optimal scale must be sought elsewhere. One possible procedure would be to approximate the appropriate scale by a process of experience and trial-and-error. It would be reasonable to start, perhaps, with the current scale of governmental support for basic research and to allocate funds by applying some standard of scientific competence and promise of scientific achievement. It should then be feasible to see how much of what has been spent has resulted in significantly important contributions to the progress of scientific knowledge. If less than the whole of it has had this effect, it would be successful and those who should be unsuccessful in their quest for support. If the current scale of expenditures for basic science support leaves some in the queue who have failed to obtain grants and who are none the less thought to show genuine promise of future scientific achievements the appropriation should be enlarged.

There is risk in such a procedure. The scale of basic science would depend upon the standards applied in selecting among applicants and in evaluating the importance of contributions to basic scientific knowledge. The procedure depends on the integrity with which consensual judgement is formed in the community of science and in the capacity of that community to distinguish between important and inconsequential contributions to the growth of scientific knowledge.